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XIV. *The Caloric Theory of Heat and Carnot's Principle.** By
H. L. CALLENDAR, M.A., LL.D., F.R.S., *Professor of
Physics at the Imperial College of Science and Technology,
S.W.*

"Difficile est proprie communia dicere."

1. *Introductory.*—It is the time-honoured privilege of a President on these occasions, in place of providing new and original material for discussion, to indulge in the retrospect or review of some branch of physical science in which he may be particularly interested. I propose to take advantage of this presidential privilege by submitting for your approval some reflections on the foundations of the science of heat. So much has been written, and still continues to be written, on this engrossing subject from the purely theoretical standpoint, that I should not venture to abuse your patience by choosing so hackneyed a theme if I did not feel that, by spending the best part of my life in the practical teaching and experimental investigation of the science, I had earned the right to an expression of opinion. I feel sure that, however, you may disagree with my way of putting things, you will at least believe that it is the outcome of practical experience, and represents the point of view of a serious experimentalist. The subject chosen is so large and the time at my disposal so limited that I must be content with the merest outline of a suggestion, and must leave you to fill in the details yourselves. It is impossible to cover the whole ground, or to refer to a tithe of the ideas or theories which have been from time to time proposed for developing the fundamental principles of the science. If I should appear to have omitted many considerations of importance I trust you will give me the benefit of the doubt, and believe that the omission is due merely to exigencies of space or time over which even a physicist has but a limited control.

The caloric theory of heat is now so long forgotten that we rarely hear it mentioned, except as an example of primeval ignorance; but it was not really quite so illogical as it is

* Presidential Address, February 10, 1911.

generally represented to be. The science of heat might have been developed along the lines of the caloric theory nearly, if not quite, as well as on the lines of the dynamic theory. Some most important and fundamental ideas, which are obscured in the early stages of the present method of exposition, would, in fact, have been brought into greater prominence and more widely appreciated, and a clearer view of the whole subject might thus have been obtained.

The original conception of caloric as a subtle imponderable fluid, capable of flowing from one body to another, of expanding bodies, and of producing rise of temperature or change of state, was, in the main, merely a picturesque analogy, invented, like the electric or magnetic fluids, with the object of giving the mind a tangible something to reason about, a measurable entity remaining constant in various transformations or exchanges. The fluid terminology remains a convenient method of expression even if the thing measured has not all the properties of a material fluid.

The first serious difficulties which the theory encountered were in explaining the apparent generation of heat by friction or compression. It might have been boldly assumed, as Cavendish and others maintained, that heat was generated by friction in the same kind of way as electricity; but a more generally acceptable explanation at the time appeared to be that some of the latent heat was ground or squeezed out of the bodies concerned and became sensible. The abraded or compressed matter, according to this view, was not capable of containing so much heat as the original stuff, or its "capacity for heat" was diminished. As there were no means of measuring the total heat content of a body, the "thermal capacity" was generally regarded as being proportional to the quantity of heat required to raise the temperature one degree, in which sense the phrase is still employed. The possibility of this explanation of the production of heat by friction was negatived at an early date by the experiments of Rumford and Davy. Rumford ("Phil. Trans.," 1798) in one of his experiments using a boring machine with a blunt tool, succeeded in raising 26.5 pounds of cold water to the boiling point by means of friction in $2\frac{1}{2}$ hours with the production of only 4,145 grains of metallic powder. He then showed that the metallic powder required the same quantity of heat to raise its temperature 1 deg. as an equal weight of the original metal, or that its "capacity for heat" in this sense was unaltered. He argued

that so small a quantity of powder could not possibly account for all the heat generated, and that the supply of heat appeared to be inexhaustible. Heat could not, therefore, be a material substance, but must be something of the nature of motion, as Newton and Bacon had surmised. The argument was not quite conclusive, because he was unable to show that the powder really contained the same total amount of heat as the solid metal. But the experiments must have satisfied every reasonable philosopher of the extreme improbability of the explanation then generally accepted. Davy's experiment (described in an essay "On Heat, Light and Combinations of Light," 1799), in which he melted two blocks of ice by rubbing them together, was more conclusive, since as he remarks "it is a well-known fact that the capacity of water for heat is greater than that of ice, and ice must have an absolute quantity of heat imparted to it before it can be converted into water. Friction consequently does not diminish the capacities of bodies for heat." In stigmatising the "last eleven words" as "a lame and impotent conclusion," Lord Kelvin (*Ency. Brit.*, "Heat," 1882) inadvertently does Davy an injustice, because Davy evidently uses the phrase "capacity for heat" in both senses, as was common at the time. In a later passage of the same essay he defines capacity for heat as the total thermal content of the body, and employs the unfamiliar phrase "capability of temperature" to denote the heat required to raise the temperature. 1 deg.

Although it was clear from these and similar experiments that caloric was actually generated by friction and could not be regarded as a material fluid in the ordinary sense, inasmuch as it did not in all cases obey the law of the conservation of matter, then recently established, the idea of heat as "a mode of motion" was at that time too vague to afford a satisfactory substitute for the fluid theory. Davy speaks of heat as "repulsive motion," and distinguishes it from light, which is "projective motion," and is capable of forming compounds with ordinary matter. Thus oxygen gas is not a simple substance, but a compound, termed by Davy "phosoxygen," of light and oxygen. A theory of this kind could not be usefully applied until it was shown how the motion corresponding to heat should be measured (whether by its momentum or *vis viva*, or otherwise), how much motion was required to generate a given quantity of heat, and whether the ratio of equivalence between heat and motion suitably measured was in all cases

invariable. These questions were, in fact, so difficult to settle that we can hardly wonder that it was 50 years before a satisfactory answer was obtained.

2. *Thermal Properties of Gases.*

Light was first thrown on the subject by investigations bearing on the thermal properties of gases. The laws of expansion of gases and the elementary properties of vapours were already familiar about this time from the researches of Dalton, Gay Lussac and others; but the data regarding the specific heats of gases were very meagre and conflicting. Dalton had found a rise of 50°F . produced by compressing air suddenly to half its volume. This was less than a third of the true rise of temperature, but in any case the result could not be interpreted in terms of quantity of heat generated without a knowledge of the specific heat.

The first reliable results for the specific heats were obtained shortly afterwards by Delaroche and Bérard ("Ann. Chim.," 1813). Their constant pressure method was a great advance on all previous work, and gave the mean specific heats referred to water between 15°C . and 100°C . They found that equal volumes of the permanent gases H_2 , N_2 , O_2 and CO at atmospheric pressure had nearly the same thermal capacities as air, the specific heat of which they found to be 0.267 calorie per gram at a pressure of 740 mm. of mercury. The compound condensable gases, CO_2 , N_2O and C_2H_4 , were correctly found to have larger thermal capacities per unit volume than air. They were unable to state whether the specific heats increased or diminished with temperature, but from an experiment with air at 1,000 mm. pressure, which gave the value 0.245 calorie per gramme, they concluded that the specific heats diminished with diminution of volume. The experiments of Regnault 40 years later showed that this observation was incorrect, but it appeared at the time to lend support to the view that the rise of temperature observed on suddenly compressing a gas was due to the diminution of its specific heat, a hypothesis otherwise unsupported by experimental evidence.

It was evident that the elasticity, or resistance of a gas to compression, would be greater in a rapid or adiabatic compression, on account of the rise of temperature, than in a slow compression at constant temperature. Laplace was the first

to see in this fact the probable explanation of the long-standing discrepancy between the observed velocity of sound and that calculated by Newton on the basis of Boyle's law of isothermal compression. According to Boyle's law, a reduction of 1 per cent. in volume at constant temperature raised the pressure of air by 1 per cent. The observed value of the velocity of sound required that in a rapid compression of 1 per cent. the pressure should be increased 1.41 per cent. This result is more accurately expressed by the statement that the ratio of the adiabatic to the isothermal elasticity of air is 1.41. At a later date ("Ann. Chim.," 1816) Laplace stated that he had succeeded in proving that the ratio of the adiabatic to the isothermal elasticity of a substance was the same as the ratio of the specific heat at constant pressure to the specific heat at constant volume. This important result follows directly from the definitions of the two elasticities and the two specific heats, and is independent of any view as to the nature of heat or temperature. But it appeared at the time to afford the strongest support to the caloric theory, because it showed that the specific heat was actually greater when the volume was allowed to increase than when the volume was kept constant, and because no explanation was forthcoming on the kinetic theory. The specific heats measured by Delaroche and Bérard were those at constant atmospheric pressure. All attempts to measure the specific heats at constant volume had signally failed on account of experimental difficulties, but their values could be calculated from the known value of the ratio thus deduced by Laplace.

A few years later Clément and Désormes ("Journ. de Phys.," 1819) succeeded in verifying the value of the ratio deduced from the velocity of sound by directly measuring the changes of pressure produced by adiabatic and isothermal compression. They found the value 1.354, which was lower than the true value 1.41, because the compression in their experiments was not strictly adiabatic. Gay Lussac and Welter ("Ann. Chim.," 1822) repeated the experiment with slight improvements, employing expansion instead of compression, and found a rather better approximation, namely, 1.375. They found the ratio practically constant for a range of pressure from 144 to 1,460 mm., and for a range of temperature from -20°C. to $+40^{\circ}\text{C.}$ Assuming on this evidence the constancy of the ratio of the specific heats of air, Laplace ("Œuvres," V., p. 143) showed that the specific heat per unit volume at a pressure p

should vary as $p^{1/\gamma}$, and the specific heat per unit mass as $p^{1/\gamma-1}$, according to the caloric theory, where γ is the ratio of the specific heats. This result happened to agree precisely with the values found by Delaroche and Bérard for air at 740 and 1,000 mm., and appeared to be an additional confirmation of the caloric theory. The same propositions were demonstrated by Poisson ("Ann. Chim.," 1823, XXIII., p. 337), who also gave the familiar relation, $p v^\gamma = \text{constant}$, between the pressure and volume of a gas in adiabatic expansion or compression. The last equation is evidently correct, but the reasoning with regard to the variation of the specific heat with pressure is somewhat obscure, and the result given does not appear to follow necessarily from the caloric theory.

3. Carnot's Reflexions on the Motive Power of Heat.

The greatest step in advance was made about this time by Carnot in his famous essay, entitled "Reflexions on the Motive Power of Heat," Paris, 1824. Proposals had frequently been made to employ other agents, such as air, alcohol or ether, in place of steam in a heat engine, with a view to securing higher efficiency or a greater quantity of motive power for a given consumption of fuel. Carnot proposed to find the answer to the questions, how the efficiency was limited, and whether other agents were preferable to steam. In considering this problem he first points out that motive power (which he measures in kilogrammetres, and employs as equivalent to the modern term "work") cannot be said to be produced from heat alone, unless nothing but heat, or its equivalent fuel, is supplied. All parts of the engine, including the working substance or agent, must be at the end of the process in precisely the same state as at the beginning. He here assumes a fundamental axiom, which had always been taken for granted, but not so precisely and explicitly stated by previous writers.

Carnot's Axiom for a Cyclical Process.

"When a body has undergone any changes and after a certain number of transformations is brought back identically to its original state, considered relatively to density, temperature and mode of aggregation, it must contain the same quantity

of heat as it contained originally.”* The ideal engine for theoretical purposes must, therefore, be supposed to work in a complete cycle of operations in which everything is restored to its initial state, except that a certain quantity of heat has been supplied to and utilised by the engine. This does not seriously limit the application of the theory, because, although practical engines do not always use the same identical mass of working substance for each stroke, they necessarily repeat a regular cycle of operations continuously, and it is generally possible to devise an equivalent theoretical cycle in which the working substance is restored after each stroke to its initial state. Carnot proceeds to show that motive power cannot be obtained from heat by alternate expansion and contraction of the working substance, without postulating the existence of two bodies at different temperatures to serve as boiler and condenser, or source and sink of heat respectively; and conversely that, wherever a difference of temperature exists, it is possible to utilise it for the production of motive power. He thus deduces the fundamental rule for obtaining the best results.

* Carnot goes on to say: “In other words, the quantities of heat absorbed or disengaged in its diverse transformations are exactly compensated. This fact has never been doubted; it has been first assumed without reflection, and then verified by calorimetric experiments. To deny it would be to upset the whole theory of heat, for which it serves as a basis. For the rest, one may say in passing, the principal foundations on which the theory of heat rests, require the most attentive examination. Many experimental facts appear almost inexplicable in the present state of this theory.” Clausius “Pogg. Ann.,” 1850; “Phil. Mag.,” IV., vol. II., p. 2, 1851) misquotes this passage most ingeniously. He fails to notice that the essential point assumed by Carnot in any cyclical process is that the quantity of heat remaining in the body is the same when it has been brought back identically to the same state. He makes Carnot “expressly state that no heat is lost in the process, that the quantity (transmitted from the fireplace to the condenser) remains unchanged.” Clausius makes *this* “the fact which has never been doubted,” &c. In reality, Carnot, in describing his cycle, does not make the assumption which Clausius here attributes to him, because it is not a necessary part of the reasoning. Carnot does not, even in the paraphrase of his axiom, say that the quantities of heat absorbed or disengaged in its diverse transformations are *equal*, but merely that they are *exactly compensated*, so far as the body itself is concerned. Whether they are equal or not depends on the way in which a quantity of heat is measured. The essential point of the reasoning in Carnot’s description of his cycle is that the result is independent of any assumptions with regard to the way in which temperature or heat are measured, provided that the quantity of heat remaining in the body is the same when it has been brought back to its original state after any transformations. The special word “compensated,” which Carnot employs in place of “equal” in paraphrasing his axiom, would appear to imply that he did not consider the quantities of heat absorbed and disengaged necessarily equal, and that this was one of the points which “required the most attentive examination.”

Carnot's Criterion of Thermal Reversibility.

"In order to realise the maximum effect, it is necessary that, in the process employed, there should not be any direct interchange of heat between bodies at sensibly different temperatures." Direct transference of heat between bodies at different temperatures would be equivalent to wasting a difference of temperature which might have been utilised for the production of motive power. This rule excludes all the well-recognised thermal sources of waste of power, which practical engineers had already been most successful in reducing. The ideal engine was also assumed to be free from mechanical losses such as friction. Under these conditions there is equilibrium, both mechanical and thermal, at every stage of the process, so that each operation is reversible, and may be supposed to be performed in either direction at will.

4. Carnot's Ideal Cycle.

Carnot first gives a rough illustration of an imperfect cycle, using steam in much the same way as in an ordinary steam engine, but supposing most of the usual losses absent. After expansion to condenser pressure, the steam is supposed to be completely condensed, and then returned as cold water to the hot boiler. He points out that the last stage does not conform to his rule for maximum effect, because, although the water is restored to its initial state, there is direct passage of heat from a hot body to a cold body in the last process. Having thus established the general notions of a perfect cycle, and of reversibility as the criterion of perfection, he proceeds to give a more exact illustration, employing a gas as the working substance. In this demonstration he assumes only the well-established experimental facts (1) that a gas may be heated or cooled by rapid compression or expansion, and (2) that, if compressed or expanded slowly in contact with conducting bodies, it may be made to evolve or absorb heat at constant temperature. Carnot's cycle itself is so familiar that it is scarcely necessary to describe it in detail. It is much to be regretted, however, that it is not always given as nearly as possible in Carnot's own words. His description is so nearly perfect that it could hardly be improved. Details which may appear superfluous at a first perusal are seen, on more careful study, to be exactly in the right place. Unnecessary assumptions have been introduced by others in describing the cycle, and have led to mistakes

which Carnot carefully avoided. For instance, Clapeyron, in describing the cycle, directs one to compress the gas at the lower temperature in contact with the cold body B until the heat disengaged is equal to that which has been absorbed at the higher temperature. This operation would be very difficult to perform, and involves the unnecessary assumption that the gas at this point contains the same quantity of heat as it contained at the beginning of the first stage, so that when the body B is removed, the gas will be restored to its original temperature by adiabatic compression to its original volume. It was chiefly for this reason that Clausius, who was acquainted with Carnot's work chiefly through the medium of Clapeyron's summary, so completely misinterpreted it, and substituted another proof. The same mistake undoubtedly induced Lord Kelvin to state ("Phil. Mag." IV., 1852) that "Carnot's original demonstration utterly fails," and was the cause of the "corrections" attributed to James Thomson and Clerk Maxwell respectively. In reality, Carnot's original description is independent of any assumption as to the nature of heat, and requires no correction.

After completing the description of the cycle, and showing its exact reversibility, Carnot proceeds to say: "The impossibility of producing by the agency of heat alone a quantity of motive power greater than that which we have obtained in our first series of operations, is now easy to prove. It is demonstrated by reasoning exactly similar to that which we have already given. The reasoning will have in this case a greater degree of exactitude; the air of which we made use to develop the motive power is brought back at the end of each cycle of operations precisely to its initial state, whereas this was not quite exactly the case for the vapour of water, as we have already remarked." Carnot considered the proof too obvious (as indeed it is) to be worth repeating. Unfortunately his original demonstration, referring to an imperfect cycle given merely *comme un aperçu*, as an introduction to the method, is not so exactly worded that exception cannot be taken to it. I will, therefore, repeat the proof in a slightly more definite and exact form, as Carnot probably intended it to run. "If it were possible to produce from a given quantity of heat supplied a greater quantity of motive power than that obtained from a reversible engine, it would suffice to divert a portion of this power to return to the source by means of a reversible engine the quantity of heat taken from it. We should thus obtain at each repetition of the cycle a balance of motive power without

taking any heat from the source—that is to say, without any consumption of fuel.” The extreme improbability of such a result is a sufficient *reductio ad absurdum* to satisfy any reasonable intelligence. Whether the condenser is heated or cooled in the process is immaterial for practical purposes. The condenser might be the ocean or the whole earth. We should thus obtain the practical equivalent of a perpetual motion, interpreted by Carnot as the continuous creation of motive power. If the conservation of energy is assumed, it is true that the condenser would be cooled. But it hardly seems necessary to invoke the law of the conservation of energy, and to make a final appeal to axioms, such as those of Kelvin* or Clausius, which are far less self-evident. The conservation of energy as applied to heat is itself an experimental law, and the final appeal must be to experiment in any case.

5. Carnot's Principle.

Carnot stated his principle as follows :—

“ The motive power of heat is independent of the agents set at work to realise it ; its quantity is fixed solely by the temperatures of the bodies between which in the limit the transfer of heat is effected.”

He adds to this statement : “ It is necessary to understand here that each of the methods of developing motive power attains the perfection of which it is susceptible. This condition will be fulfilled if, as we have remarked above, there is not produced in the body any change of temperature which is not due to a change of volume, or, what is the same thing otherwise expressed, if there is never contact between bodies at sensibly different temperatures.” He also explains that the principle applies equally to different substances and to different states of the same substance, provided that the temperature limits are the same for the agents compared in any case.

Carnot's principle may be stated in a great variety of different ways, which are more or less equivalent, but it seems best to adhere as closely as possible to Carnot's own words, the mean-

* Carnot's proof does not assume either that heat is measured as energy or that energy is conserved. If both these points are assumed, his axiom that it is impossible to make a heat engine work without taking heat from the source, or without consumption of fuel, is equivalent to Kelvin's axiom that motive power cannot be obtained by cooling the condenser. One may be deduced from the other; but Carnot's axiom is the most direct result of experience with heat engines, involves the fewest assumptions, and appears to be the most appropriate for the required formal proof.

ing of which is sufficiently clear and exact for all practical purposes. Whatever form of words is adopted, it will always be necessary to define the terms employed, and to explain how the principle is to be applied. On this understanding many different modes of expression are equally valid and satisfactory but many paraphrases of Carnot's principle have been given from time to time which appear to be distinctly objectionable as obscuring the real points at issue. Clausius, for instance ("Pogg. Ann.," 1850, Vol. LXXIX., p. 369) quotes Carnot's principle as follows: "The performance of work is equivalent to a transference of heat from a hot body to a cold body without the quantity of heat being thereby diminished." This method of statement omits both the essential points, (1) that the efficiency is independent of the working substance, and (2) that it is a function solely of the temperature limits. It also introduces an unnecessary assumption with regard to the manner in which a quantity of heat is measured, which is not an essential part of Carnot's reasoning. The principle itself is independent of the manner in which either heat or temperature is measured. Provided that the methods of measuring heat and temperature are consistent, and are the same for all substances compared, the only effect of altering the temperature scale or the definition of the heat unit, is to alter the form of the function representing the efficiency.

6. Carnot's Applications of his Principle.

Carnot endeavoured to apply his principle to the greatest possible variety of cases, and to subject it to the most complete experimental verification, so far as the data available at that time would permit. He succeeded in establishing for the first time several important results, which have in some cases been overlooked or attributed to others. He appears to have worked out most of these results analytically in the first instance as indicated by his footnotes, and to have subsequently translated the formulæ into words in the text for the benefit of his non-mathematical readers. He expressed his principle analytically in the following simple form. If W is the quantity of work or motive power obtained in a reversible cycle from a quantity of heat, Q ,* communicated to the working substance at a temperature, t , C, the condenser, being assumed for convenience at 0°C ., we may write

$$W/Q = F(t), \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* It is not here assumed that heat is necessarily measured as energy.

where $F(t)$ is some function of the temperature limits, depending on the way in which temperature and heat are measured but independent of the properties of the working substance. Owing to the limited state of knowledge with regard to the physical properties of bodies at that time, he was unable to apply the principle generally in this form. He points out that a great simplification is introduced in the application of the principle by considering a cycle of infinitesimal range, dt , but finite heat absorption Q , at a temperature t . He thus obtains by differentiation the corresponding expression,

$$dW/dt = QF'(t), \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $F'(t)$ is the derived function of $F(t)$, and is generally known as Carnot's function. This function evidently denotes the quantity of work obtainable per degree fall in a Carnot cycle per unit of heat supplied at the temperature t ; and is the same for all substances at the same temperature.

Carnot, in the first instance, by applying the principle in this form to the discussion of the thermal properties of gases, endeavoured to complete the solution of the problem from the theoretical side by showing how the value of his function $F(t)$ or $F'(t)$ might be expected to vary with temperature, consistently with the ascertained laws of gases, and with various simple assumptions made with regard to properties which had not been experimentally determined.

7. *Properties of Gases deduced by Carnot from his Principle.*

Carnot's discussion will be followed as closely as possible with a few slight changes in the notation to render it more familiar. The work done in isothermal expansion at a temperature, T (Carnot writes $t+267$) by a gas obeying the law $pv=RT$, in expanding from v_0 to v is $RT \log_e (v/v_0)$. The work done in a cycle of range dT per 1 deg. fall is evidently $dW/dT = R \log_e (v/v_0)$. By Carnot's principle this is equal to $QF'(t)$, where Q is the corresponding quantity of heat absorbed. We thus obtain the expression for the heat absorbed by a gas in isothermal expansion,

$$Q = R \log_e (v/v_0)/F'(t), \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which must be the same for all gases at the same temperature for the same ratio of expansion v/v_0 , provided that they obey the law $pv=RT$, and that corresponding quantities of the different gases are taken (equal volumes at the same standard

temperature and pressure) so that the constant R is the same for all. In other words, the heat absorbed by a gas in isothermal expansion is proportional to the whole work done, and bears to it the ratio $1/TF'(t)$. This result is independent of the way in which temperature and heat are measured, provided that the gases obey the same law of isothermal expansion and give the same scale of temperature.

Since, when a gas absorbs a quantity of heat equal to the difference of the specific heats at constant pressure and volume, the work done in isothermal expansion is $p v/T$ or R , Carnot deduces from the above general result that the difference of the specific heats (for equal volumes measured under the same conditions) must be the same—namely, $R/TF'(t)$ —for different gases at the same temperature, and must also be independent of the pressure. He draws up a table showing the values of the specific heats at constant volume deduced from the results of Delaroche and Bérard at constant pressure. On this he remarks that the *ratio* of the specific heats must be different for different gases, whereas it had generally been assumed to be the same. The elevation of temperature produced by a sudden compression must therefore be different for different gases, and he gives a table showing the results.

Since, according to the experiments of Gay Lussac and Welter the ratio of the specific heats of air is independent of the pressure and temperature, and according to his own theory the difference of the specific heats must also be independent of the pressure at the same temperature, it follows that the specific heats themselves must be independent of the pressure. Carnot points out that this is inconsistent with the result of Delaroche and Bérard previously cited for the specific heat of air at 1,000 mm. pressure—namely, 0.245—which is less than the value 0.267 found at 740 mm. The discrepancy must be due to errors of experiment. The variation of the specific heats with pressure was doubtful, because it rested on a single difficult experiment, and the range of pressure was too restricted for a satisfactory conclusion to be drawn.

8. Carnot's Application of the Caloric Theory.

The results so far obtained by Carnot in his investigations with regard to the specific heats of gases, were independent of the caloric theory and remain equally true on the kinetic theory, although they have frequently been credited to other

writers.* Because in some cases he employed the language and reasoning of the caloric theory, it does not follow, as most writers have hastily assumed, that his whole argument breaks down. In attempting to unravel the relations of the specific heats, Carnot was compelled to employ some theory or assumption as to the nature of heat. He naturally adopted the caloric theory, because no other theory was available at that time, and the experimental data were too inadequate to permit of any other method of treatment.

The most fundamental postulate peculiar to the caloric theory is that the quantity of caloric required to be added to a body to transform it from any state, defined, we may suppose, by temperature t_0 and specific volume v_0 , to any other given state, defined by temperature t and specific volume v , is the same, in what ever way the change is effected. Carnot applies this assumption to the problem in hand in the following manner. Suppose that unit mass of gas is first heated at constant volume v_0 up to the temperature t , absorbing a quantity of caloric U_0 , and is then expanded at constant temperature t , absorbing a quantity of caloric $R \log_e (v/v_0) F'(t)$, as proved in the last section. Next suppose that the gas is first expanded from v_0 to v at constant temperature t_0 , absorbing a quantity

* Dulong ("Ann. de Chim.," XLI., p. 156, 1829), adopting a method due to Chladni, compared the velocities of sound in different gases by observing the pitch of a note given by the same tube when filled with the gases in question. He thus obtained the ratios of the specific heats. For O_2 , H_2 and CO the ratios were the same as for air. But for CO_2 , N_2O and C_2H_4 the ratios were much smaller than for air. On comparing his results with the specific heats for the same gases found by Delaroche and Bérard, Dulong observed that the changes of temperature for the same compression were in the inverse ratio of the specific heats at constant volume. He thus deduced the important conclusion, agreeing with that obtained theoretically by Carnot five years earlier, that "equal volumes of all gases under the same conditions evolve on compression the same absolute quantity of heat." Carnot's result is more general and significant, and it is clear that he deserves some credit for the prediction.

Joule in 1845 showed by experiment that the ratio (Carnot's $TF'(t)$) of the work done to the heat evolved in compressing a gas was nearly, if not quite, the same as the ratio J of work spent in heating water by friction to the heat evolved. The mechanical equivalent J must, therefore, be equal to Carnot's $TF'(t)$, if heat is measured as energy. Since Joule's experiments were all performed practically at one temperature, they do not afford direct evidence as to the variation of $F'(t)$ with temperature, unless we assume that a quantity of heat is measured as a quantity of energy which cannot vary with temperature. Admitting this, $F'(t)$ must be equal to J/T , as Joule himself suggested in a letter to Kelvin in 1848. Carnot is entitled to some credit for having predicted that the ratio must be constant at any one temperature, or must be a function of the temperature only. Mayer in 1842 assumed the equivalence on the basis of the old principle "*causa aequal effectum*," which could hardly be regarded as scientific demonstration.

of caloric given by $R \log_e(v/v_o)/F'(t_o)$, and is then heated at constant volume v from t_o to t , absorbing a quantity of caloric U . The total quantity of caloric Q required in either case must be the same on the caloric theory. Whence we have,

$$U_o + R \log_e(v/v_o)/F'(t) = Q = U + R \log_e(v/v_o)/F'(t_o). \quad (4)$$

From this equation it is immediately obvious that, if the specific capacity for caloric at constant volume is independent of the volume, since $U = U_o$, $F'(t)$ must be equal to $F'(t_o)$; or Carnot's function must be independent of the temperature, if the latter is measured on the gas scale and if the heat is measured as caloric.* We also observe, as Carnot points out, that it is not necessary for the specific capacity for caloric to be a function of the volume in order to explain the rise of temperature produced by compressing a gas. But this assumption had so frequently been made by Laplace and others that it had come to be regarded as an essential part of the explanation of the phenomenon on the caloric theory. Carnot also investigates the general law of the variation of specific heat with pressure, admitting the result of Delaroche and Bérard as correct. But since we know now, from the observations of Regnault, first published in 1852, that the result of Delaroche and Bérard was wrong, and that the specific heat of air is practically independent of the pressure, it is clear that Carnot deserves some credit for having given the correct solution of the problem on this assumption.

Putting $F'(t) = \text{constant} = A$, Carnot obtains for the work

* The corresponding expression for $F'(t)$ on the mechanical theory may be obtained by an exactly similar process. If a quantity of heat is measured as energy, the difference of the quantities of heat absorbed and rejected in the cycle represented by equation (4)—namely, $U_o - U + R \log_e(v/v_o)[1/F'(t) - 1/F'(t_o)]$ —must be equal to the external work done in the cycle—namely, $R \log_e(v/v_o)(T - T_o)$ —divided by the mechanical equivalent J . If the specific heat at constant volume is independent of the pressure $U = U_o$, and $F'(t)$ must be equal to J/T . Substituting this value of $F'(t)$ in Carnot's expression (3) for the heat absorbed by a gas in isothermal expansion, we see that it is equivalent to the work done provided the gas obeys Boyle's law, and that its specific heat at constant volume is independent of the pressure. Clausius at a later date reversed this procedure. He began by assuming the equivalence of the heat absorbed to the work done by a gas in isothermal expansion, and deduced that the specific heat was independent of the pressure. But Carnot's method appears more logical and direct. The assumption of the equivalence of heat and work in a non-cyclical process, thus made by Mayer and Clausius, was, in effect, a violation of Carnot's fundamental axiom, and could not be justified theoretically. Carnot's assumption is also preferable from the experimental standpoint, because it proposes a more direct and simple test—namely, that the specific heat should be the same at different pressures.

done per unit of caloric supplied at a temperature $t^{\circ}\text{C.}$ in a finite cycle of range t to 0°C. , the simple expression $F(t)=At$. Expressing the same result in terms of temperature T on the gas scale, we obtain for the work, W , due to a quantity, Q , of caloric supplied at a temperature, T , in a Carnot cycle of range T to T_0 , the expression

$$W=AQ(T-T_0). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This expression is less general than equation (1), because it assumes that a quantity of heat is measured as caloric, and that temperature is measured on the scale of a gas obeying Boyle's law and possessing the property that its specific heat at constant volume is independent of the pressure. Admitting these assumptions the solution is obviously correct. The quantity of caloric, Q , supplied at the temperature, T , is necessarily the same as the quantity rejected at the temperature T_0 , because the quantity of caloric in the working substance must remain constant, when no heat is supplied, in reversible expansion or compression such as is postulated by Carnot in his cycle. We observe that, if heat is measured as caloric, it is quite correct to state that "the production of work from heat is due, not to an actual consumption of caloric, but to the transport of caloric from a higher to a lower temperature." Carnot's analogy of the water-wheel is thus exactly justified. Caloric, like water, is not itself motive power, but is capable of performing work under suitable conditions in virtue of its tendency to flow down a temperature gradient. The quantity of motive power produced is directly proportional to the quantity of caloric and to the fall of temperature.

9. *The Motive Power of a Quantity of Caloric.*

The solution thus found by Carnot, and expressed in equation (5), was not merely the correct final solution of the problem on the caloric theory, it is also immediately obvious that it is perfectly consistent with the kinetic theory of heat, and supplies the master key to the relations between heat and motive power. Caloric may be said to possess motive power in virtue of its elevation of temperature, just as water possesses motive power in virtue of its available head or pressure. In the case of caloric there is a definite limit set by the absolute zero of temperature to the quantity of motive power obtainable by a cyclical process from a quantity of caloric supplied at any temperature. Putting $T_0=0$, the absolute zero, the maximum

quantity obtainable from a perfect gas as working substance, and therefore from any other substance whatever, is evidently AQT , which may be regarded as the absolute work-value of a quantity of caloric, Q , supplied at a temperature, T , on the absolute scale. The quantity of motive power developed, if the fall available is $T - T_0$, is $AQ(T - T_0)$. The limit of efficiency for such a fall, or the ratio of the motive power obtained to the total motive power of the caloric supplied, is $(T - T_0)/T$. The motive power of the caloric rejected at T_0 is AQT_0 . The sum of the quantities of motive power utilised and rejected is equal to the total motive power of the quantity supplied at T .*

Carnot's solution is not merely consistent with the mechanical theory; it directly states the principle of the conservation of motive power, and gives all the results which follow from that theory so far as they relate to reversible processes. He could not fairly have been expected to realise this at the time and to express his results in terms of the mechanical theory, because that theory had not been put in a definite form. His results are not necessarily incorrect because they were expressed in terms of the caloric theory. The numerical values which he calculated in various ways for the motive power of caloric were correct so far as the experimental data available at that time permitted. They deserve in reality to be regarded as the first calculations of the mechanical equivalent of heat, although they were expressed in a manner which renders them at first sight unfamiliar to modern readers.

10. Carnot's Numerical Verification of his Principle.

The investigation of the specific heats of gases having failed, on account of discordant experimental data, to lead to a *conclusive* result with regard to the variation of $F'(t)$ with tem-

* This result, which is equivalent to the fundamental postulate of the mechanical theory, may readily be extended to a reversible cycle of any form. The given cycle may be supposed to be divided by a family of adiabatic or equicaloric curves into a number of elementary Carnot cycles of different ranges. For each of these elementary cycles, the quantity of caloric absorbed is equal to that rejected, and the difference of the energies of the caloric supplied and rejected is equal to the external work done. Taking the sum of the elementary cycles, the total quantity of caloric absorbed must be equal to that rejected in any cycle, which is the fundamental postulate of the caloric theory; and the difference of the quantities of heat energy absorbed and rejected in the cycle must be equal to the external work done, which is the fundamental postulate of the mechanical theory. The two postulates, so far from being opposed to each other, are mutually equivalent, and both are implied in Carnot's solution.

perature, Carnot next devotes his attention to the numerical calculation of the values of $F'(t)$ at different temperatures from the properties of different gases and vapours, so far as the scanty material at his disposal permitted. His first calculation from the properties of air at 0°C . appears at first sight to be unnecessarily long and involved, owing to his elaborate endeavour to avoid what he calls algebraic language; but he probably deduced the answer directly in the first instance from his expression for the heat absorbed in isothermal expansion, applied to the difference of the specific heats—namely, $R/TF'(t)$. Employing his data, the value of $F'(t)$, or the work done in a cycle per degree fall per unit of heat supplied, comes out 1.395 kilogrammetres per kilocalorie per degree fall at 0°C . This method of calculation, based on the work done in a cycle, does not, like that of Mayer or Clausius, involve the assumption that the *whole work* done in isothermal expansion is equivalent to the heat supplied, which is true only for a perfect gas. Carnot's method applies to any gas or vapour, however imperfect, and would give the same result exactly for all substances at the same temperature if the experimental data were correct. The absolute equivalent of the kilocalorie is obtained, as explained in the last section, by multiplying this result by the absolute temperature corresponding to 0°C . on the scale of a perfect gas—namely, 273—which gives 380 kilogrammetres for the mechanical equivalent of the kilocalorie. The discrepancy from the true value, 427 kilogrammetres per kilocalorie, is due merely to errors in the experimental data.

For steam at 100°C ., taking Dalton's value of (dp/dt) , the rate of increase of pressure with temperature—namely, 26 mm. of mercury, or 0.36 metre of water—and taking the specific volume of steam at 100°C . as 1,700 litres per kilogramme, Carnot finds the motive power dW/dt per degree fall in a cycle employing 1 kg. of steam, to be $1,700 \times 0.36 = 611$ kilogrammetres. The quantity of heat supplied is the latent heat of vaporisation per kilogramme at 100°C ., which he takes as 550 kilocalories. The work in kilogrammetres per kilocalorie per degree fall, or the value of $F'(t)$ is

$$F'(t) = 611/550 = 1.112 \text{ at } 100^{\circ}\text{C}.$$

Multiplying this result by the absolute temperature 373° , we obtain 415 kilogrammetres as the absolute equivalent of the kilocalorie, which is a better approximation, because the data for steam were more accurate than those for air.

The method of calculation employed for steam is equivalent to the formula

$$(v-w) (dp/dt) = LF'(t), \quad (6)$$

where $(v-w)$ is the change of volume on vaporisation, and L the latent heat. This formula is generally known as Clapeyron's, but Carnot appears to be entitled to some credit for it, because it is simply the direct expression of his principle as applied to change of state, and because he was the first to employ it for numerical calculation, although he does not happen to give the formula itself in algebraic form in his foot-notes.

Applying the same calculation to the vapour of alcohol at its boiling-point, $78.7^{\circ}\text{C}.$, Carnot found the value $F'(t) = 1.230$ kilogrammetre per kilocalorie per degree fall. This would not necessarily agree with the value found from steam at $100^{\circ}\text{C}.$, because the temperature was different. Assuming, in accordance with Watt's law, that the total heat of steam reckoned from $0^{\circ}\text{C}.$ was constant and equal to 650 kilocalories per kilogramme, the latent heat at $78.7^{\circ}\text{C}.$ would be 571 kilocalories. Taking the appropriate value of (dp/dt) from Dalton's tables, Carnot thus finds $F'(t) = 1.212$ from steam at $78.7^{\circ}\text{C}.$, which agrees very fairly with the result deduced from alcohol at the same temperature. A similar calculation for steam at $0^{\circ}\text{C}.$, taking the latent heat as 650 from Watt's law, gave the value $F'(t) = 1.290$, which differs materially from the value 1.395 found for air at the same temperature; but the data for steam were here so uncertain, that no stress could be laid on the discrepancy. The value $F'(t) = 1.290$ gives 352 kilogrammetres for the kilocalorie. The discrepancy in the values of the kilocalorie deduced in this way from steam at different temperatures is not to be wondered at, because Lord Kelvin ("Trans." R.S. Edn., XIV., 1849) in re-calculating by a modification of Carnot's method the mechanical equivalent J from Regnault's greatly improved data for steam, found values ranging from 1,357 ft.-pounds per pound degree C. at $0^{\circ}\text{C}.$ to 1,578 ft.-pounds at $230^{\circ}\text{C}.$ Joule had then by direct experiment found values ranging from 1,368 to 1,476 ft.-pounds at $10^{\circ}\text{C}.$ to $16^{\circ}\text{C}.$ Lord Kelvin was not satisfied that the value of J was the same at different temperatures. It required a bold speculator like Clausius to argue that there could be no variation with temperature.

Carnot laments that data were lacking, especially the values

of (dp/dt) , to enable him to calculate $F'(t)$ for solids and liquids, and particularly for the case of fusion of ice. If he had been in possession of Lord Kelvin's value—namely, 130 atmospheres—of the pressure required to lower the freezing point of water 1°C ., he would have found $F'(t)=1.53$, or 418 kilogrammetres as the equivalent of the kilocalorie at 0°C .. With the data at his disposal, one can hardly wonder that he was baffled at every turn in his attempts to verify his theory.

Reviewing the numerical data for $F'(t)$, which varied from 1.112 kilogrammetres per kilocalorie per degree fall at 100°C ., to 1.290 and 1.395 at 0°C ., Carnot was led to the conclusion that $F'(t)$ probably diminished with rise of temperature on the gas scale, but that the numerical data were not sufficiently exact to determine the law of variation with temperature. Unfortunately the apparent diminution of $F'(t)$ with rise of temperature was most readily explained by supposing that the specific heat of a gas was not independent of the volume, but increased with increase of volume, as Delaroche and Bérard had found. We see immediately from Carnot's equation (4) that if v is greater than v_0 , and U greater than U_0 , then $F'(t)$ must be less than $F'(t_0)$. In other words, $F'(t)$ must diminish with rise of temperature. It happened that the mistake made by Delaroche and Bérard was very nearly of the right order of magnitude to account for the observed rate of diminution of $F'(t)$ with rise of temperature. Carnot was thus dissuaded from pursuing further the simple and obvious solution—namely, that $F'(t)$ was constant—which he had founded on the assumption that the specific heats were independent of the pressure.

If Carnot had not been misled by this fundamental error in the work of Delaroche and Bérard, and if his numerical data had been a little more accurate, he could not have failed to notice that the quantity of caloric in a kilocalorie varied inversely as the absolute temperature, or that its equivalent motive power was the same at all temperatures. In other words, that the kilocalorie was a unit of motive power, and not a unit of caloric. According to his data, neither the work obtainable per degree fall, nor the total work obtainable from a kilocalorie, was the same at different temperatures. Both appeared to vary to about the same extent, but in opposite directions. The point which naturally did not occur to Carnot, and which the experimental data were too uncertain to suggest, was the extremely fundamental point (which was also missed by subsequent writers in dealing with the caloric theory)

that a quantity of caloric as measured by his equation, did not remain constant under the conditions of an ordinary calorimetric experiment, when heat was allowed to flow directly from a hot body to a cold body.

11. *Measurement of Caloric.*

According to Carnot's solution, $W=AQ(T-T_0)$, the natural measure of a quantity of caloric is the work done per degree fall in a Carnot cycle.

The constant A in this relation may be reduced to unity by a suitable choice of units. The absolute unit of caloric is that quantity which is capable of doing unit work per degree fall in a Carnot cycle. If the absolute unit of work is taken as the joule or watt-second, the absolute unit of caloric may appropriately be styled the CARNOT.

The CARNOT is that quantity of caloric which is capable of producing one joule of work in a Carnot cycle per 1°C . fall on the scale of a perfect gas.

The number of carnots of caloric required to vaporise one gramme of water at 100°C . under standard conditions is readily calculated from Carnot's data. Since the work done per gramme vaporised per degree fall is 0.611 kilogrammetres, or nearly 6 joules, the caloric of vaporisation is nearly 6 carnots. The work done per degree fall per kilocalorie of heat supplied at 100°C . is 1.112 kilogrammetres, or nearly 11 joules. The number of carnots of caloric in a kilocalorie at 100°C . is nearly 11. Taking 130 atmospheres C.G.S. as the pressure required to lower the melting point of ice 1°C ., and 0.092 cubic cm. as the diminution of volume per gramme on melting, the work obtainable from 1 gramme of ice at 0°C . in a Carnot cycle per degree fall is $130 \times 0.092/10 = 1.2$ joules. The caloric of fusion is, therefore, 1.2 carnots. The mechanical equivalent of Q carnots supplied at T Abs. is QT joules. These values are calculated from the work which might be done in a reversible cycle, and are quite independent of calorimetric data. The caloric absorbed by 1 gramme molecule of a perfect gas ($R=8.3$ joules per 1 deg.) in expanding at constant temperature from v_0 to v , is given by Carnot's equation (3) as $Q=8.3 \log_e(v/v_0)$ carnots, and is independent of the temperature. The difference of the specific capacities for caloric at constant pressure and volume at a temperature, T , is $8.3/T$ carnots per degree or the absolute work value of the difference is 8.3 joules per degree.

12. *Equivalence of the Caloric and Kinetic Theories.*

The exact correspondence between the solution obtained by Carnot on the caloric theory with that subsequently obtained by Clausius, Rankine and Kelvin on the kinetic theory, is most easily appreciated if we compare the steps by which each solution was obtained. For this purpose we may follow the exposition of the kinetic theory as given by Clausius, as it is the most direct, and corresponds most nearly with the method of Carnot. Clausius starts with assuming Carnot's relation in the form $dW/dt = QF'(t)$, which is equally true on either theory, whether heat is measured as energy or as caloric. The divergence begins with the assumption that heat is measured either as a quantity of caloric (denoted by Q)*, or as a quantity of energy (denoted by q). Adopting the gas scale of temperature, these assumptions determine the form of the function $F'(t)$.

Carnot shows, as already explained, that if the specific heat of a gas at constant volume is independent of the pressure, $F'(t)$ must be constant, $=A$, on the caloric theory, if temperature is measured on the gas scale. This gives $dW/dt = AQ$, or $W = AQ(t - t_0)$ in a finite cycle. The measure of caloric is work per degree fall, and the work equivalent of caloric is found by multiplying the caloric by the absolute temperature and by a constant A . It follows that the work done by a perfect gas in isothermal expansion is the equivalent, AQT , of the caloric supplied, and that the difference of the specific capacities for caloric R/AT , is the same for all gases if equal volumes are taken.

Clausius proceeds in the opposite direction. He begins by assuming with Mayer as highly probable on general grounds that the work done by a gas in isothermal expansion is the equivalent Jq of the heat absorbed, which gives immediately, from Carnot's equation, $F'(t) = J/T$ when heat is measured as a quantity of energy. This had been previously suggested by Holtzmann (1845), but Clausius, having the advantage of Joule's experiments, was able to show that the numerical values of J/T were all reasonably consistent with Carnot's values of $F'(t)$ for the calorimetric unit. Applying this assumption to gases, it follows that the specific heat at constant volume must be independent of the pressure, and that the difference of the specific heats must be equal to R/J . Clausius

* To indicate that a quantity of heat is measured differently in the two sets of formulæ, capitals will be used for quantities depending on the caloric measure of heat, and small letters for the corresponding symbols in energy measure.

ignores the evidence of Delaroche and Bérard on the variation of the specific heat with pressure, which gave Carnot so much trouble, but adopts that of Gay Lussac and Welter on the constancy of the ratio, which he (erroneously) states "cannot be reconciled with the theory of Carnot as heretofore treated." Assuming the constancy of the specific heats, he deduces Poisson's equation for the adiabatics. He finally obtains for the heat absorbed by a gas in isothermal expansion from v_0 to v the expression $q = RT \log_e (v/v_0)/J$, which is the same as the assumption with which he started, and is equivalent to Carnot's expression (3). The results thus obtained were not new, except in the manner of expression. They were given to show that the kinetic theory was consistent with the then ascertained properties of gases.

We have already seen that Carnot's principle takes the form given in equation (6) when applied to vapours. Substituting $F'(t) = A$, this becomes $(v-w)(dp/dt) = AL$, where L is the latent caloric. On the kinetic theory we must substitute $F'(t) = J/T$, and the equation becomes $(v-w)(dp/dt) = Jl/T$, where l is the latent heat energy, and is equal to the latent caloric L multiplied by T , if the units are chosen so that the constants A and J are the same. Assuming J constant, Clausius employs this equation (in the reverse way to Lord Kelvin) to calculate the deviations of steam from the ideal state, which he regards as being the explanation of the variation of J with temperature deduced by Lord Kelvin from Regnault's experiments. This calculation was well meant, but indecisive, because, as we now know, Regnault's formula for the latent heat was erroneous.

Joule, by his experiments on gases, had arrived at the conclusion that in the performance of a heat engine the heat energy converted into work was not given back to the condenser. This is true of heat measured as energy, and appeared at first not only to be a conclusive disproof of the caloric theory, but also to be quite irreconcilable with the proof of Carnot's principle as given by Clapeyron. Clausius showed that this result was not necessarily inconsistent with Carnot's principle itself; but that, on the kinetic theory, in any cyclical process, the heat returned to the condenser must be less than that taken from the source by a quantity equivalent to the external work done. Applying this principle to an infinitesimal cycle with steam as the working substance, he deduced a new and unexpected result, discovered about the same time by Rankine. If saturated steam is compressed, heat must be abstracted

from it to reduce it to the state of saturation at the higher pressure and temperature; in other words, the specific heat of steam maintained in the state of saturation is negative. This result is so good an illustration of the difference between the kinetic and caloric theories that it deserves fuller explanation.

If s' , s'' are the saturation specific heats of water and steam respectively, l and $l+dl$ the latent heats of vaporisation at neighbouring temperatures T , and $T+dT$, the heat supplied in raising unit mass of water from T to $T+dT$ and vaporising it at $T+dT$ will be $s'dT+l+dl$. The heat abstracted in cooling the saturated steam to T , and condensing it at T , will be $s''dT+l$. The difference of the quantities of heat absorbed and abstracted—namely, $(s'-s'')dT+dl$ —must, according to the kinetic theory, be equivalent to the work done in the cycle $(v-w)(dp/dt)dT$. But this work by the previous equation (6) deduced from Carnot's principle, is itself equivalent to a quantity of heat $(l/T)dT$. Dividing the equation by dT , so as to obtain the difference in the quantities of heat absorbed and abstracted per degree range of the cycle, we obtain the well-known equation of Clausius,

$$s'-s''+dl/dT=l/T. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Knowing the approximate values of all the quantities except s'' at 100°C .—namely, $s'=1$, $dl/dT=-0.70$, $l/T=540.373=1.45$ calories per degree—we obtain with Clausius, $s''=s'+dl/dT-l/T=-1.15$, or the specific heat of saturated steam 100°C . is negative and numerically greater than that of water. Clausius remarks that this is inconsistent with the caloric theory, which gives the formula $s''=s'+dl/dT$, without the negative term l/T , and would make the specific heat *positive* and equal to $+0.30$, according to Regnault's data. In reality there is no inconsistency. It is merely a question of the difference between the caloric measure and the energy measure of heat.

On the caloric theory the corresponding equation is deduced as follows by Carnot's method. If S' , S'' are the specific capacities for caloric of water and steam in the state of saturation, and if L and $L+dL$ are the values of the latent caloric of vaporisation at T and $T+dT$, the caloric absorbed in the cycle—namely, $S'dT+L+dL$ —must be equal to the caloric abstracted—namely, $S''dT+L$ —we thus obtain

$$S''=S'+dL/dT, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which agrees *in form* with the equation ascribed by Clausius to the caloric theory, but the quantities involved are quan-

tities of caloric, and not quantities of energy. If we imagine s' , s'' and l , expressed in joules, and S' , S'' and L in caloric, the latter are reduced to joules by multiplying them by T . Multiplying through the last equation by T , and writing $S''T=s''$, $S'T=s'$ and $L=l/T$, we obtain

$$s''=s'+Td(l/T)/dT=s'+dl/dT-l/T, \quad . . . \quad (9)$$

which is *identical* with the equation deduced by Clausius from the kinetic theory.

✱ We deduce from the above example, or from many others which might be given, that the fundamental postulate of the caloric theory, which Carnot employed in deducing his solution $W=AQ(T-T_o)$, and which Clausius was regarded as having conclusively disproved—namely, that the caloric absorbed is equal to that abstracted in any reversible cycle—so far from being opposed to the kinetic theory, is mathematically equivalent to Joule's proposition that the heat energy absorbed exceeds that abstracted by the equivalent of the work done.

It is surprising that Clausius in his first Paper did not give the complete solution for a Carnot cycle of finite range, which follows immediately from his assumption $F'(t)=J/T$ on the kinetic theory. Putting $H=Jq$ for the heat absorbed at T , measured in work units, and observing that dH , the difference between the quantities of heat absorbed and abstracted, is equal to dW the work done in the cycle, Carnot's equation $dW/dt=qF'(t)$ reduces to the form, $dH/dt=H/T$. From which it immediately follows that the ratio H/T of the heat H taken in at T to the temperature T in a finite cycle of range T to T_o , is the same as the ratio H_o/T_o of the heat rejected H_o to the temperature T_o at which it is rejected. Since $H-H_o=W$, the work done in the cycle, the solution may be written in the familiar form,

$$H/T=H_o/T_o=W/(T-T_o), \quad . . . \quad (10)$$

which is precisely equivalent to Carnot's solution, $Q=Q_o=W/(T-T_o)$.

The solution in something approaching very nearly to this form appears first to have been given by Rankine, without proof, in a footnote to his Paper on the Mechanical Theory of Heat ("Phil. Mag.," Ser. IV., Vol. II., p. 65, 1851). He gives "the maximum value of the fraction of the whole heat converted into expansive power" as $(T-T_o)/(T-k)$, "where k is a constant, the same for all substances." which is apparently intended to take account of the difference between the absolute

zero of the gas thermometer, and the "absolute zero of heat." Lord Kelvin ("Phil. Trans.," 1854) subsequently proceeded by the inverse process. Instead of adopting the scale of a perfect gas, as assumed by Carnot, he defined absolute temperature, by the above relations (10), in such a way as to agree with the perfect gas scale; and proceeded to investigate, with Joule's assistance, the deviations of actual gases from the scale so defined.

13. *Reappearance of Caloric in the Mechanical Theory.*

Since Carnot's equation, $dW/dt = QF'(t)$, was adopted without material modification into the mechanical theory, so that $QF'(t)$ remained simply a quantity of Carnot's caloric (though Q was measured in energy units and $F'(t)$ received the appropriate value J/T required to reduce energy units to caloric), it was inevitable that caloric should make its reappearance sooner or later in the mechanical theory. It first appears, disguised as a triple integral, in Kelvin's solution ("Phil. Mag.," IV., p. 305, 1852) of the problem of finding the work obtainable from an unequally heated body. The solution (as corrected later) is equivalent to the statement that the total quantity of caloric remains constant when the equalisation of temperature is effected by means of reversible engines. Caloric reappears next as the "thermodynamic function" of Rankine, and as the "equivalence-value of a transformation" of Clausius ("Pogg. Ann.," XCIII., p. 497, 1854). Finally, in 1865, when its importance was more fully recognised, Clausius ("Pogg. Ann.," CXXV., p. 390) gave it the name of "entropy," and defined it as the integral of dQ/T . This definition depends on the calorimetric or energy measure of heat, and obscures the fact that the caloric measure of heat follows directly from Carnot's principle, and may be made independent of the calorimetric measure. No one at that time appears to have appreciated Carnot's solution, or to have realised that entropy was merely caloric under another name. In justice to Carnot, it should be called caloric, and should be defined directly by means of his equation $W = AQ(T - T_0)$. This method of procedure appears to be justifiable both logically and historically, and leads to a more practical and definite conception of entropy or caloric as the true measure of a quantity of heat as opposed to a quantity of thermal energy. The mathematical definition of entropy, as the integral of dQ/T under certain restrictions, is unintelli-

gible to the average student, for whom the conception of entropy possesses an artificial atmosphere of unreality. The conception of caloric defined by Carnot's equation would be more readily assimilated, and might be introduced at a much earlier stage. The direct investigation of the properties of caloric thus defined would probably lead to a more general and intimate appreciation of Carnot's principle itself and of many modern developments of thermodynamics which are practically beyond the comprehension of the majority of students under the present system.

14. *Caloric in Irreversible Processes.*

Carnot was the first to distinguish between reversible and irreversible processes in thermodynamics, and to lay down the simple and sufficient rules for the reversibility of a heat engine. (1) Such an engine must work without friction, which involves direct waste of mechanical power, and had long been recognised as the non-conservative element in mechanical systems. (2) There must be no direct passage of heat from a hotter to a colder body, which is the criterion of thermal reversibility. In order to complete the caloric theory of heat, it was necessary to find an answer to the questions : (1) What becomes of motive power spent in friction ? and (2) What happens to caloric when it passes directly from a body at a higher to a body at a lower temperature ?

Rumford and Davy had proved beyond cavil that caloric was generated by friction. Carnot (as we see from the notes published by his brother) had already, before his early death in 1832, arrived at the general theory of the equivalence of heat and motive power, and had projected a number of experiments in which the motive power consumed should be measured at the same time as the heat produced. From the purely theoretical side it would have been most natural to assume that the absolute motive power of the caloric generated was equal to the motive power consumed, as given by the equation $W=AQT$, for the maximum amount of motive power obtainable from a quantity Q of caloric at T , which with A for $F'(t)$ is the same as Carnot's expression for the heat evolved in compressing a perfect gas. We know now that this is the correct solution. But Carnot, being aware of the conflicting nature of the experimental evidence, felt that further experimental verification was necessary, which was first afforded by Joule.

Carnot showed that the direct passage of heat from a hotter to a colder body was equivalent to wasting a difference of temperature which might have been utilised for the production of motive power. It was natural to ask what became of the motive power wasted. In the analogous case of the waterfall, the answer is fairly obvious. If the water is allowed to fall freely, its motive power is expended in the production of *vis viva* equivalent to its fall, which is converted into heat by friction when its motion is arrested. Similarly, when a compressed gas is allowed to expand into a vacuum, the work which might have been obtained from it by reversible expansion in a cylinder provided with a piston, is spent in producing *vis viva* of the issuing current of gas, which is finally converted into heat by friction, so that the quantity of caloric in the gas is increased, instead of remaining constant as in reversible adiabatic expansion. Carnot considers a case of this kind in a footnote. Gay Lussac and Welter had observed that if a small opening were made in a large reservoir of compressed air, and the bulb of a thermometer were presented to the issuing current, there was no sensible lowering of temperature observed, such as would have been produced in a similar reversible expansion. Carnot attributes this result partly to the development of caloric by friction against the sides of the orifice, and partly to the increase of pressure close to the thermometer bulb due to the impact.

When a quantity of caloric, Q' , supplied at a temperature, T' , is allowed to fall directly by conduction to a temperature T'' , without producing the equivalent motive power $AQ'(T'-T'')$, which might have been obtained from the fall ($T'-T''$) by means of a reversible engine, it is not so immediately obvious, owing to our ignorance of the mechanism of conduction, what becomes of the motive power wasted. The case is analogous to the fall of a quantity of electricity in a conductor, through a difference of potential in the conductor, in which case the caloric generated is equivalent to the motive power wasted. On Weber's hypothesis the carriers of heat and electricity in metals are the same, and the methods by which caloric is generated in either case must be closely related. It would be reasonable to assume by analogy that a quantity of caloric, $Q'(T'-T'')/T''$, equivalent to the wasted motive power at the lower temperature, was generated in the fall by some process analogous to friction. Adding this to the original quantity Q' , we find the quantity Q'' of caloric recovered at the lower temperature T'' equal to $Q'T'/T''$. In other

words, the quantity of caloric is increased by falling freely in such a way that its equivalent motive power $AQ''T''$ at the lower temperature is the same as that which it originally possessed—namely, $AQ'T'$ —at the higher temperature. This is precisely what we now assume on the analogy of material systems in applying the principle of the conservation of energy to heat. Carnot appears to have foreseen this result, and to have devised experiments for verifying it, but so far as I know it has not been explicitly verified, merely because it has not occurred to anyone to define caloric explicitly in terms of work per 1 deg. fall in a cycle, and to compare this measure directly with the calorimetric units.

At the present time so many data exist for the verification of the increase of caloric in an irreversible process, that it is unnecessary to perform a special experiment. The inevitable result of such an experiment may be predicted by a simple calculation, such as the following. We have already seen that the caloric of vaporisation of steam at $100^{\circ}\text{C}.$, measured in terms of Carnot's equation, is approximately 6 carnots per gramme, and the caloric of fusion of ice at $0^{\circ}\text{C}.$ is 1.2 carnots per gramme. Arrange an experiment in which steam is condensed at $100^{\circ}\text{C}.$ on one side of a conducting partition, while ice is melted at $0^{\circ}\text{C}.$ on the other side, without allowing any heat to escape. Measure both the steam condensed and the ice melted in a given time. We know, as the result of innumerable calorimetric experiments of this kind, that for each gramme of steam condensed, $540/79.5$ grammes of ice approximately would be melted. Six carnots at $100^{\circ}\text{C}.$ when allowed to fall directly to $0^{\circ}\text{C}.$ by conduction, produce $540 \times 1.2/79.5 = 8.17$ carnots at $0^{\circ}\text{C}.$ The quantity of caloric at $0^{\circ}\text{C}.$ is greater than that supplied at $100^{\circ}\text{C}.$ in the proportion of 8.17 to 6, which is nearly the same as the ratio $373/273$ of the absolute temperatures. The motive power of the caloric at $100^{\circ}\text{C}.$ —namely, $6 \times 373 = 2,238$ joules—is the same as the motive power of the caloric found at $0^{\circ}\text{C}.$ —namely $817 \times 273 = 2,231$ joules—within the limits of error of the data. Similarly in other cases, whenever available motive power is wasted “in the useless re-establishment of the equilibrium of caloric,” an equivalent quantity of caloric is generated, so that the total motive power, including any useful work done, remains constant. In a reversible cycle, when there is no waste and the efficiency is a maximum, the total quantity of caloric remains constant. The increase of caloric, if any, due to

friction or conduction, is a measure of the motive power wasted.

Caloric is not conserved in the old sense that its total quantity remains constant in any system of exchanges under all conditions. It remains constant only in the restricted sense, provided that all the exchanges are reversible or take place under conditions of equilibrium. But since in any irreversible exchanges the total quantity of caloric is always increased, the principle of the conservation of caloric can still be stated in the modified form that "the total quantity of caloric in a system *cannot be diminished* by any internal reactions or by the performance of work by or on the system, provided that no heat is allowed to escape." Applied in this form, with various modifications to suit special conditions, it has proved to be one of the most fruitful principles of modern thermodynamics; but it might have been reached sooner, and more generally appreciated, if the caloric theory had not been so hastily abandoned.

14. *The Calorimetric Measure of Heat.*

The only defect of the caloric theory as developed by Carnot lay in the tacit assumption that the ordinary calorimetric units were units of caloric. This, no doubt, was one of the fundamental points in the theory of heat, which, in Carnot's opinion, "required the most attentive examination." He had himself supplied the key to the difficulty in his criterion of thermal reversibility. But no experimental data were available to decide the point. The pioneers of the mechanical theory made the same tacit assumption when they took it for granted that a given quantity of work done in compressing a gas at any temperature would produce the same number of units of heat in a calorimeter. Granting that heat was measured as energy in a calorimeter, and that energy was conserved, the required result followed; but this was the very point which was to be proved. Even Joule's experiments were not sufficient in themselves to decide the point, because they were all performed at nearly the same temperature, so that it made no difference whether heat was measured as caloric or as energy. According to Carnot's equation (3), the ratio of the caloric evolved to the work done should be constant at any one temperature, and should vary comparatively slowly with the temperature. Lord Kelvin appears to have appreciated this point when he endeavoured to calculate, by means of Regnault's data for steam, the number

of foot-pounds of work required to generate 1 calorimetric unit of heat by compressing a gas at different temperatures. The number varied from 1,357 to 1,578 foot-pounds per pound-degree C., but this result was not conclusively against the mechanical theory, because, as Clausius showed, it admitted a different interpretation. The first experiments which were sufficiently definite and accurate to decide the point were those of Regnault ("Phil. Mag.," V., p. 473, 1853), which showed that the specific heat of air, measured calorimetrically, was nearly independent of pressure and temperature over a wide range. It follows from Carnot's expression $R/TF'(t)$ for the difference of the specific heats, that if heat is measured calorimetrically, $F'(t)$ must be equal to J/T as required by the mechanical theory, assuming the ratio of the specific heats to be constant. But if Carnot's solution (deduced on the assumption that the specific heat of air was independent of the pressure) had not been overlooked, it would have been obvious that caloric itself was not energy, and that Regnault's result was not inconsistent with the measure of heat as caloric, but only with the tacit assumption that the caloric measure was identical with the calorimetric measure. If this had been recognised at the time, it would have been quite unnecessary to recast and revolutionise the entire theory of heat. Evolution would have proceeded along safer lines with the retention of caloric, and the investigation of its properties, which are so important in all questions of equilibrium in physics.

15. *Caloric the True Measure of Heat Quantity.*

Since a quantity of heat is measured most directly as a quantity of thermal energy in an ordinary calorimetric experiment, we have become so saturated with the idea that heat is energy and must be measured in units of energy, that we are apt to forget that a quantity of heat is not completely specified by its energy equivalent. The absurdity would be at once apparent, to take an analogous case, if we were to measure a quantity of electricity always in kilowatt-hours. The equivalent energy determines the cost of production, and is for many purposes the appropriate and sufficient measure, but we should fare very badly in electrical theory without the separate units of quantity in ampere-hours and pressure in volts. In electricity, the conditions of practical measurement have led us naturally to units of quantity defined in terms of electric and magnetic

forces, and the unit difference of potential follows as that difference through which unit quantity must fall in doing unit work. In the science of heat Carnot's equation provides us with a precisely analogous measure of heat quantity or caloric as distinct from a quantity of energy. The unit of caloric is that which does unit work in falling reversibly through 1 deg. of temperature. The unit of caloric might be defined directly in terms of the expansion of a gas at constant temperature by the equation $Q = R \log_e (v/v_0)$, in which case the scale of temperature would follow. But since this method would be inconvenient for the practical measurement of caloric, we define the temperature scale first, as that of the perfect gas, and deduce the natural unit of heat quantity, which comes to the same thing. It is true that we can solve most questions in heat in terms of energy and temperature, without explicit reference to caloric or its equivalent. We could similarly solve most electrical problems without mentioning amperes. But since caloric possesses the important property, essential to the natural measure of heat quantity, of remaining constant in reversible exchanges (which the energy measure of heat does not) all our equations and methods of reasoning with regard to questions of equilibrium are greatly simplified and rendered more direct if we adopt caloric as the true measure of heat quantity, and regard it as possessing energy in virtue of its temperature. With all our preconceived notions of heat as a "form of energy," it is difficult now to retrace our steps and express everything in terms of caloric. But, if Carnot's solution had not been overlooked 60 years ago, it would have seemed equally absurd to regard a quantity of heat merely as a quantity of energy, and we should probably have been far ahead of our present position.

16. *The Material Nature of Caloric.*

The objection most commonly urged against the caloric theory from the earliest times has always been that it was absurd to suppose that anything which could be generated without limit could possibly be regarded even in thought as a material fluid. Some 20 years ago, the fluid theory of electricity was regarded as being equally illogical and physically unsound, although it was generally retained on account of the many useful analogies which it suggested. A natural reaction, consequent on fresh discoveries, has caused the pendulum to

swing far in the opposite direction. Electricity is now more material than ever, and is continually invading other branches of physics with materialistic notions. So long as a quantity of heat is regarded as being necessarily a quantity of energy, the fluid conception is of little or no use. It is true that a quantity of energy cannot be created or destroyed, but since it may be transformed into an equivalent quantity of any other kind of energy, it cannot be identified as remaining constant in any one state. This objection does not apply to caloric. It is true that caloric can be created without limit, just as we think and speak of electricity as being generated; but it has the most important attribute that, when once generated, it can never, so far as we know, be destroyed. Moreover, caloric remains constant under precisely those limiting conditions of equilibrium which are most important for purposes of practical calculation. It is true that it may be difficult to isolate a particular set of material particles and label them caloric. It is conceivable that caloric may not be material at all, but merely, as Boltzmann puts it, "the logarithm of the probability of a complexion." But if this is really the case, it is all the more necessary for our sanity and progress to think and speak of it as a material fluid. Although this conception of caloric may appear at first sight to run counter to some of our most cherished popular illusions with regard to heat, there could be no serious objection to adopting it as a convenient method of expression. The more shadowy the conception to be visualised, the greater the need of a definite material analogy. From this point of view the old picturesque phraseology of the material fluid, implied in Carnot's waterfall, so far from being a valid objection, is one of the chief advantages of the caloric theory.

17. *Conclusion.*

The modes of thought and expression inherent in the caloric theory lead most naturally and inevitably to the conception of the conservation of caloric as the fundamental law peculiar to thermodynamics. To the mathematician, who loves to deal in abstract mysteries, the substitution of the crude idea of caloric for the esoteric conception of entropy may doubtless prove repellent; but to the experimentalist, who prefers to think in concrete realities, the change of view point cannot fail to be suggestive. So long as entropy was merely a mathematical abstraction it was unnecessary to attach any definite meaning

to the absolute quantity of entropy in a body, and no one objected to its being infinite when reckoned from zero pressure, temperature, or volume. But if we regard caloric as the natural measure of heat quantity, and push the conception to its logical conclusion, the absolute quantity of caloric in a finite body must be finite, and must vanish at the absolute zero of temperature. The specific heats in energy measure, instead of remaining constant or tending to a finite limit, as generally assumed, must tend to vanish as the zero is approached. The ideal gas of constant specific heat is an obvious objection to this rule, but the real gas avoids the difficulty by condensing in good time. This direct corollary of the caloric theory, so far from being inconsistent with experiment, is daily receiving confirmation from recent researches. It would be impossible, in a sketch like the present, to follow the caloric theory into all its possible developments, but enough has been said to show that the conception of caloric, as employed by Carnot, was not so misleading or erroneous as is generally supposed.

ABSTRACT.

The caloric theory of heat as developed by Carnot in his famous "Reflexions on the Motive Power of Heat" (Paris, 1824) leads immediately to the correct solution of the relations between heat and motive power (energy or work) in all reversible processes, and appears to be in some respects preferable to the mechanical theory as a method of expression, because it emphasises more clearly the distinction first clearly stated by Carnot, between reversible and irreversible transformations, and because it directly provides the natural measure of a quantity of heat as distinct from a quantity of thermal energy.

Carnot first introduced the method of the cyclical process in discussing the action of a heat engine, and showed that, in the ideal case, if there were no direct transference of heat between bodies at different temperatures, the transformations of heat and motive power in such a cycle were reversible. Assuming that it was impossible to imagine a heat engine capable of producing motive power perpetually without taking any heat from the boiler, he concluded that the quantity of motive power, W , produced from a given quantity of heat, Q , by means of a reversible engine, working between given temperature limits in a cyclical process, was the maximum obtainable; or that the efficiency must be independent of the agents employed, and must be a function of the temperature limits alone. He expressed this by the equation $W/Q = F(t)$, between finite limits 0° and $t^\circ\text{C}$., or by the equivalent equation $dW/dt = QF'(t)$ for a cycle of infinitesimal range, dt , at a temperature, t , where $F'(t)$ (generally known as Carnot's function) is the derived function of $F(t)$, and must be the same for all substances at the same temperature.

Applying the equation in this form to a gas obeying the law $pv = RT$, he showed that the heat absorbed in isothermal expansion from v_0 to v

was given by the expression $Q = R \log_e(v/v_0)/F'(t)$, and that the difference of the specific heats at constant pressure and volume, given by the expression $S_p - S_v = R/TF'(t)$, must be independent of the pressure, and the same for equal volumes of all gases. These results were new, but were confirmed experimentally by Dulong five years later. Carnot showed, further, that if the ratio S_p/S_v was constant (as found by Gay Lussac and Welter, and assumed by Laplace and Poisson), both S_p and S_v must be independent of the pressure.

The results so far obtained by Carnot, including the description of his reversible cycle and the deduction of his fundamental principle, were independent of any assumption as to the nature of heat. Applying the assumption of the caloric theory, that the quantity of caloric required to change the state of a substance from (v_0, t_0) to (v, t) was the same by any reversible process, Carnot deduced that, if S_v was independent of the pressure, the function $F'(t)$ must be constant, $= A$. This assumes that heat is measured as caloric, and that temperature is measured on the scale of a gas, obeying the law $p v = RT$, and having S_v independent of the pressure, which is equivalent to the modern definition of a perfect gas. Putting $F'(t) = A$, he obtains for the work W produced from a quantity of caloric, Q , supplied at a temperature, T , in a cycle of finite range T to T_0 , an expression equivalent to the following :—

$$W = A Q (T - T_0).$$

Carnot was unable to reconcile this solution with the imperfect experimental data available in his day, and particularly with the observation of Delaroche and Bérard, supported by Laplace's theory, that the specific heat of air, S_p , diminished with increase of pressure, which we know now, from the experiments of Regnault, to have been incorrect. He therefore made no serious attempt to apply the solution, and subsequent writers have apparently failed to observe that it is the correct final solution of the problem on the caloric theory. With our present knowledge, it is easy to see that this solution of Carnot's is also consistent with the mechanical theory, and contains implicitly all the relations of heat and work so far as they relate to reversible processes. The quantity, Q , of caloric remains constant in reversible expansion such as is postulated by Carnot, when no heat is supplied. The work done is directly proportional to the temperature range $T - T_0$. The absolute motive power or equivalent work-value of a quantity of caloric, Q , supplied at a temperature, T , is the maximum work obtainable from a perfect gas (and therefore from any other substance whatever) when $T_0 = 0$, namely, AQT . The efficiency of the cycle with range T to T_0 is $W/AQT = (T - T_0)/T$. The external work done in the cycle is the difference of the work-values of the caloric supplied and rejected, a result which is readily extended to cycles of any form.

To complete Carnot's solution, it is necessary to enquire what happens to caloric in irreversible processes, such as friction, or the direct passage of heat from a hotter to a colder body. Carnot, as we see from his posthumous notes, had already, before his early death in 1832, arrived at the general conception of the conservation of motive power, and had planned experiments in which the motive power consumed in friction should be measured at the same time as the caloric generated. According to his theory, it would have been natural to assume that the motive

power of the caloric generated at any temperature, namely AQT , should be equal to the motive power consumed in friction. But he realised that further experimental evidence was necessary, which was first supplied by Joule.

A quantity of caloric is defined in Carnot's equation as measured by work done in a Carnot cycle per degree fall. The absolute unit of caloric, which may appropriately be called the CARNOT, is that quantity which is capable of doing one joule of work per degree fall. The mechanical equivalent of Q carnots at T Abs. is QT joules. From Carnot's data, the work done in a cycle per gramme of steam vaporised at 100°C . per degree fall is 0.611 kilogrammetres, or nearly 6 joules. The caloric of vaporisation is 6 carnots. Similarly, from Kelvin's data for the pressure required to lower the freezing point 1°C ., the caloric of fusion of ice is 1.2 carnots. Since this definition is independent of calorimetric measurements, it may be employed in a calorimetric test, in which steam is condensed at 100°C . on one side of a conducting partition while ice is melted at 0°C . on the other, to determine by direct experiment what happens when caloric falls irreversibly by conduction from 100°C . to 0°C . We know that for each gramme of steam condensed, or for each 6 carnots supplied at 100°C ., 540.79.5 grammes of ice approximately would be melted, or 8.17 carnots of caloric would appear at 0°C . The quantity of caloric is increased in the proportion 373/273. The motive power of the caloric remains constant if no useful work is done. The increase of the quantity of caloric is the same as if the available motive power $AQ(T-T_0)$ had been developed and converted into heat by friction at the lower temperature. Whenever motive power is wasted in friction, or "in the useless re-establishment of the equilibrium of caloric," a quantity of caloric equivalent to the wasted motive power is generated. The total quantity of caloric in an isolated system remains constant only if all the transformations are reversible, in which case the motive power developed exactly suffices to restore the initial state. In all other cases there is an increase of caloric. The old principle of the universal conservation of caloric, which is true only for reversible processes, must therefore be modified as follows:—"The total quantity of caloric in any system cannot be diminished except by taking heat from it."

This principle, with various modifications to suit special cases (such as conditions of constant temperature, pressure, or volume) is immediately recognised as one of the most fruitful in modern thermodynamics. But it appeals more forcibly to the imagination of the student, if established, as roughly sketched above, by a direct investigation of the properties of Carnot's caloric.

The caloric theory is seen to be perfectly consistent with Carnot's principle and with the mechanical theory for all reversible processes. Caloric is the natural measure of a quantity of heat in accordance with Carnot's equation, if we adopt the gas-scale of temperature. The only defect of the caloric theory lay in the tacit assumption, so easily rectified, that the ordinary calorimetric units were units of caloric. The quantity measured in an ordinary calorimetric experiment is the motive power or energy of the caloric, and not the caloric itself. If this had been realised in 1850, it would have been quite unnecessary to recast and revolutionise the entire theory of heat. Evolution might have proceeded along safer lines, with the retention of caloric, and the investi-

gation of its properties, which are of such fundamental importance in all questions of equilibrium in physics.

Since Carnot's equation, $dW/dt = QF'(t)$, was adopted without material modification into the mechanical theory, and $QF'(t)$ remained simply a quantity of Carnot's caloric (though Q was measured in energy units and $F'(t)$ received the appropriate value J/T required to reduce energy units to caloric) it was inevitable that Carnot's caloric should make its reappearance sooner or later in the mechanical theory. It first reappears, disguised as a triple integral, in Kelvin's solution ("Phil. Mag.," 4, p. 305, 1852) of the problem of finding the available work in an unequally heated body. The solution (as corrected later) is equivalent to the statement that the total quantity of caloric remains constant when the equalisation of temperature is effected reversibly. Caloric reappeared next as the "thermodynamic function" of Rankine, and the "equivalence-value of a transformation" (Clausius "Pogg. Ann.," 93, p. 497, 1854). Finally, in 1865, when its importance was more fully recognised, Clausius ("Pogg. Ann.," 125, p. 390) gave it the name of "entropy," and defined it as the integral of dQ/T . Such a definition appeals to the mathematician only. In justice to Carnot, it should be called caloric, and defined directly by his equation $W = AQ(T - T_0)$, which any schoolboy could understand. Even the mathematician would gain by thinking of caloric as a fluid, like electricity, capable of being generated by friction or other irreversible processes. Conduction of caloric is closely associated with the electrons, and the science of heat would gain, like the science of electricity, by attaching a more material conception to the true measure of a quantity of heat, as distinguished from a quantity of thermal energy.

A vote of thanks to Prof. CALLENDAR for his Presidential Address moved by Dr. CHREE and seconded by Dr. RUSSELL, was carried unanimously.

XV. *On the Movement of a Coloured Index along a Capillary Tube, and its Application to the Measurement of the Circulation of Water in a Closed Circuit.* By ALBERT GRIFFITHS, D.Sc., Head of Physics Department, Birkbeck College.

READ FEBRUARY 24, 1911.

- § 1. Introduction and elementary theory.
- § 2. Experiments with an index in a tube of 2 mm. bore.
- § 3. Details of index ; methods of observation ; and tests of consistency.
- § 4. Experiments related to the elementary theory.
- § 5. Applications.

§1. *Introduction.*

If a capillary tube, originally full of water, and along which a slow flow is taking place, be fed with a weak fluorescein solution, the point to which the colour reaches can be read, at the speeds studied in this Paper, with tolerable consistency when the illumination is constant ; and as there is a paraboloidal flow under certain conditions along a capillary tube the apex of the paraboloid moving at twice the mean speed of the water, a hasty consideration of the subject might lead to the erroneous conclusion that the extremity of the coloured index moves at twice the mean speed, or even more, the excess being due to diffusion.

At the speeds studied in this Paper, it is found that the extremity of the coloured index moves at a speed distinctly less than twice the average speed of the water ; in fact, its movement is a rough measure of the average speed.

In this Paper the full mathematical treatment is not attempted ; but an elementary consideration, although not complete, will be of advantage in dealing with the experiments. It can easily be shown that if the intensity of the colour were constant over a cross-section of the tube the colour would diffuse along the tube exactly as if the water travelled in a solid column. The intensity of the colour can not be absolutely constant over a cross-section except in the theoretical case of a capillary tube of infinitely small bore ; but the experiments of §4 show that when the rate of flow is small the error involved in the assumption is not great. By stopping the supply of fluorescein solution and replacing it by water, an approximately symmetrical column of colour of slowly increasing length can be obtained, and when the slowly moving column is at a relatively long distance from the ends of the

fine capillary, it is obvious that the movement of the centre of the column must measure the mean speed of the water.

But even when it is not correct to assume that the intensity of colour is absolutely constant over a cross-section, an approximately symmetrical column, the centre of which indicates the mean speed, can be obtained. This is proved in the next section.

§ 2. *Experiments with a Tube of about 2 mm. Bore.*

A convenient apparatus for the experimental study of the subject is shown in Fig. 1. T is a calibrated tube containing the coloured index, R is a series of fine tubes acting as a resistance, H is a tube containing water, the height of which regulates the rate of flow through the tube T, I is indiarubber

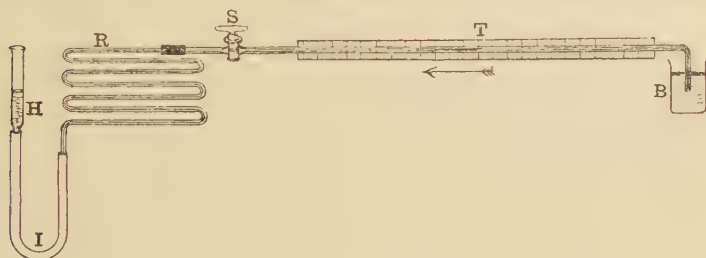


FIG. 1.

tubing, S is a stop-cock, and B is a beaker containing water on which floats a layer of oil to minimise evaporation.

The following table of observations and calculations explains itself :—

Average readings of ends of index, 1 p.m., Nov. 19th	134.32, 144.31
Corrected readings	134.44, 144.22
Mean of corrected readings	139.33 cm.
Weight of beaker, water and oil, 1 p.m., Nov. 19th	53.6800 gms.
Average readings of ends of index, 11 a.m., Nov. 24th ...	52.25, 70.02
Corrected readings	52.38, 69.927
Mean of corrected readings	61.15 cm.
Weight of beaker, oil and water, 11 a.m., Nov. 24th	50.8232 gms.
Correction due to evaporation, obtained with aid of auxiliary beaker	0.0028 gm.
Corrected weight of beaker	50.8260 gms.
Corrected loss of weight of beaker	2.854 gms.
Product of movement (78.18 cm.) of index into mass (0.4964 ÷ 13.56) per linear centimetre	2.859 gms.
Percentage difference.....	0.18
A second experiment gave :—	
Corrected loss of weight of beaker	3.8028
Product of movement (104.056 cm.) of index into mass (0.4964 ÷ 13.56) per linear centimetre	3.8099
Percentage difference.....	0.19

§ 3. *Details of Index, Methods of Observation and Tests of Consistency.*

The fluorescein solution employed in the experiments of this Paper was made by adding one quarter of a gram of fluorescein and two drops of an aqueous solution of ammonia of strength 0.88 to a litre of distilled water. The tube containing the index is placed on a black scale with white graduation marks.

When readings were taken directly with the eye the source of illumination was an arc lamp taking 12 amperes, placed in a lantern with only the condenser in position. A sheet of ground glass covered the condenser to diffuse the light and to give an even illumination of the index; and a sheet of blue glass placed in front of the ground glass absorbed a quantity of light without any appreciable diminution of the fluorescence of the index.

The consistency of the readings was tested in the case of a calibrated tube of about 0.25 mm. diameter, by introducing two indexes in series, the linear speed being of the order of 1.5 cm. per hour, or 36 cm. per day.

	Movement of front index. Centimetres.		Movement of back index. Centimetres.		Ratio of front to back.
1	55.39	55.00	1.0071
2	51.63	51.83	0.9960
3	58.27	58.22	1.0009
4	68.18	68.19	0.9999
5	68.25	68.33	0.9988
6	51.14	51.34	0.9961
7	55.94	56.21	0.9952
8	44.95	44.97	0.9996
9	52.48	52.08	1.0077
10	54.55	54.65	0.9981
11	57.47	57.39	1.0014
12	59.47	59.57	0.9983
Average					0.999925

The probable error of a single value of the ratio, as given by the formula $r=0.8453\sum v \div \sqrt{n(n-1)}$, where $\sum v$ is the sum of the arithmetical differences between the values of the ratios and 0.999925, is 0.002561—i.e., about $\frac{1}{4}$ per cent.

The probable error of the arithmetical mean of the ratios, as given by the formula $r=0.8453\sum v \div n\sqrt{n-1}$ is 0.0007392—i.e., less than $\frac{1}{10}$ per cent.

On the assumption that all the indexes are equally reliable, whatever their ages, it can be proved that the probable error of an individual reading of the mean position of an index is

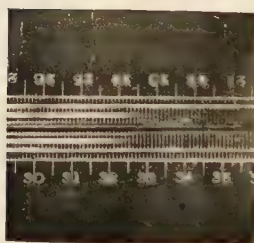


FIG. 2.

Velocity 1.25 cm. per hour; diam. min.; age 2 days.

To face p. 193.

0.7 mm. The assumption is not correct; nevertheless, the value 0.7 mm. gives a rough idea of the order of accuracy when no special devices are employed to obtain an exact reading. It must be mentioned that as a rule only one observation of each end of an index was taken.

After the preceding 12 experiments were concluded, an attempt was made to determine the consistency of the reading of the middle point of the index by making two parallel white lines on a strip of black paper at a distance apart approximately equal to the length of the index. The strip of paper was adjusted until the two white lines occupied a symmetrical position with respect to the index. Mr. H. G. Bell, who is responsible for nearly all the readings of this Paper, took six readings with the aid of the paper strip. The probable error calculated from the six readings came out to be 0.36 mm.

A photographic method of taking the readings has been developed which possesses some features of interest. The source of light is the same as that employed for the eye-readings, excepting that the blue glass is removed. Panchromatic plates are used in the camera, with a screen of "Rapid Filter Green." The arc is about 2 ft. from the tube, and the exposure corresponds to that of about 6 seconds with a lens aperture of f 11. After the negative has been developed and dried, it is cut close and parallel to the image of the capillary tube. The index is then cut into two approximately equal parts, and the two halves are adjusted until one is as nearly as possible a match of the other.

When the two halves are matched the arithmetical mean of any two corresponding readings gives the centre of the index. The three pictures in Fig. 2 elucidate the method. The uppermost shows the positive of the complete index. The left-hand figure shows the two halves matched, the right-hand figure shows an inaccuracy of matching of 2 mm., the mean reading being inaccurate by 1 mm. The figures refer to an index in a tube of about 1 mm. diameter; the index had travelled for two days at the rate of about $1\frac{1}{4}$ cm. per hour.

Mr. Bell matched the two halves six times in succession. His results were: 28.00, 28.01, 27.995, 28.00, 28.00, 28.00; mean 28.0008 cm.

Mr. F. E. Tinkler, who has frequently assisted Mr. H. G. Bell during the last 12 months, likewise took six readings. His results were: 28.00, 28.025, 27.98, 28.05, 27.96, 28.00; mean 28.0025 cm.

It may be confessed that the negative was a specially good one.

A series of seven pairs of photographs was taken of two indexes in series in a tube of $\frac{1}{4}$ mm. bore. The negatives were not good, and the probable error of an individual reading, as calculated from the series, came out about twice the probable error of a reading taken with the unaided eye; but the author is convinced that the method specified above, which is satisfactory for a tube of 1 mm. bore, is not adapted for the photographing of an index in a tube of $\frac{1}{4}$ mm. bore.

§ 4. *Experiments Relating to the Elementary Theory.*

The tube referred to in section 2 and an auxiliary tube of the same diameter, both being originally full of water, were fed with a solution of fluorescein, the speed of flow being of roughly the same value in each case. After a movement of about 15 cm. had taken place the auxiliary tube was disconnected from the flow apparatus and each end sealed with an india-rubber cap. A fiducial mark was made on the auxiliary tube. Then the auxiliary tube was placed alongside the principal tube and its position adjusted, until the ends of the columns in each tube matched, and a reading was taken of the position of the fiducial mark. The flow was allowed to continue in the principal tube, and after some days the columns were again matched and a reading taken of the fiducial mark. The flow in the principal tube was calculated (*a*) by weighing a beaker and contents, as in § 2, and (*b*) from the product of the movement of the extremity of the coloured column, as measured by the auxiliary tube, into the mass of water per linear centimetre of the tube.

In one experiment the end of the column moved at the comparatively rapid rate of about $1\frac{1}{6}$ cm. per hour through a distance of about 84 cm. It was obvious by inspection that the end of the moving coloured column was more diffused than the end of the column in the auxiliary tube; nevertheless an attempt at matching was made. The flow calculated from the movement of the coloured column as determined by the auxiliary tube was 1.4 per cent. greater than that calculated from the loss of weight of the beaker.

In a second experiment with the same tube the speed was much slower, being about $\frac{1}{3}$ cm. per hour through a range of about 89 cm. The movement as determined by method *b* was

1.04 per cent. greater than that as determined by method *a*. In this experiment there was no characteristic difference between the ends of the two columns.

An experiment was performed with a calibrated tube of $\frac{1}{4}$ mm. bore; here the weighing method of determining the flow is impracticable, and the flow was measured by placing an index in advance of the continuous column. An auxiliary matching tube of $\frac{1}{4}$ mm. diameter was employed to measure the movement of the continuous column. The speed was about $1\frac{1}{4}$ cm. per hour over a range of about 63 cm. The movement of the column as measured by the auxiliary tube was 0.75 per cent. greater than the movement of the index. The reader is reminded that the vertex of the paraboloid moves 100.00 per cent. faster than the index.

Although this section deals particularly with the flow of an indefinitely long column of fluorescein solution, a few words on the determination of the position of a finite index may not be out of place. The author is of the opinion that the best results would be given by the photographic method, if all the details were carefully arranged to suit the tube employed; or else by some equivalent optical device which would enable the two ends to be seen alongside and thus matched directly. At present, however, he would recommend the observer to be content with ordinary eye-readings taken with the assistance of a simple piece of apparatus which is a modification of the strip of black paper mentioned in § 3. It consists of two black metal pieces running along the scale at an adjustable distance apart, each metal piece carrying a vernier, the zero line of which can be brought opposite any desired point near an end of the fluorescein index.

Some experiments, similar to those in § 3, with an index moving at the speed of approximately 4 cm. per hour for about 17 hours in a tube of about 1 mm. diameter, show that with the vernier arrangement the probable error of an individual reading is 0.2 mm. This means that the probable error of a single measurement of the movement is about $\frac{1}{20}$ th per cent.

§ 5. *Applications.*

Fig. 3 gives a diagrammatic sketch of an arrangement designed for the introduction of an index into a closed circuit in which there is a circulation of water. T is a capillary tube, R is a small reservoir, A, B, C and D are taps. The ends of the tubes associated with B and C are provided with indiarubber

caps. To introduce the index close the taps A and D and run fluorescein solution in at C and out at B. Close the taps B and C, open A and D and allow the flow in the capillary to take place. When the column of fluorescein solution is of the desired length, close the taps A and D, and wash the fluorescein out of the reservoir with water. Now close B and C, and open A and D. *An emphatic warning is given against any trust in glass taps.* If there is a part of the circuit open to the atmosphere, the pressure at the tap A should be determined, and the pressure due to the fluorescein solution, or the pressure due to the water used to wash out the reservoir, should not differ much from the determined value. Otherwise the taps A and D may leak to such an extent as to materially disturb the feeble agency causing the slow circulation. When the circulation is

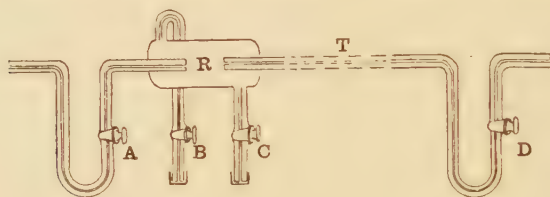


FIG. 3.

taking place, the ends of the tubes associated with B and C are covered with indiarubber caps, and all the taps are immersed in mercury contained in beakers.

A measurement is being made of the coefficient of diffusion of a salt in water by the method of diffusive convection,* and the index (Fig. 2) was obtained with an apparatus designed to apply the method. It is possible that the index may be useful in delicate thermometers and manometers.

All the work recorded in this Paper, except that connected with Fig. 2, was done in a room on the third floor of Birkbeck College. The room has two doors and three windows. It is used by research students, and no attempt has been made to keep a constant temperature. The method of causing a flow along a capillary tube described in § 2 is convenient, but it does not produce a steady flow under the conditions just mentioned, and it is probable that the indexes would be more consistent under superior conditions.

* "Proc." Phys. Soc., Vol. XVI., Part IV., pp. 230-243, Jan., 1899, or "Phil. Mag.," pp. 453-465, Nov., 1898.

In conclusion, the author desires to express his thanks to Mr. H. G. Bell, to whose skill and patience must be largely attributed the measure of success attained in the experiments described in this Paper.

ABSTRACT.

If a fine capillary tube, originally full of water, is fed with a weak fluorescein solution at a rate so rapid that the diffusion of the fluorescein may be neglected, the vertex of the coloured paraboloid moves at double the mean speed of the liquid. But at slow speeds radial diffusion plays an important part, and the colouring matter travels along the tube approximately as if the liquid moved in a solid column. If, after a short length of fluorescein solution has been introduced into the tube, the supply of solution is replaced by water, a symmetrical coloured column of slowly increasing length is obtained, the centre of which indicates the mean speed of the liquid. The illumination is the light from an arc-lamp which has been transmitted through blue glass, and the capillary tube is placed on a black scale with white graduation marks. It is comparatively easy to determine the mean speed of the liquid to an accuracy of $\frac{1}{8}$ th per cent.

DISCUSSION.

Dr. A. RUSSELL gave some illustrations of the extreme slowness of diffusion and remarked that he was sorry the author had not given more mathematics in his Paper.

The PRESIDENT remarked that very few realise the extreme slowness of diffusion. He had once worked out a case of a vertical column of a solution under the action of gravity, and found that the time required to pass from a state of uniform density to the final state of equilibrium was well over a million years.

XVI. *On an Optical Lever of High Power suitable for the Determination of Small Thicknesses and Displacements.*
By E. H. RAYNER.

READ FEBRUARY 24, 1911.

[ABSTRACT.]

THE apparatus shown arose from the necessity of measuring small thicknesses of mica. Something more sensitive than the ordinary micrometer was required. Such a micrometer magnifies about 70 times, 0.01 mm. being about 0.7 on the barrel. The optical levers used magnify satisfactorily about 10,000 times.

The principle of the tilting table was adopted, as used, for instance, in the Ewing extensometer for wires. In this apparatus a "tilting base" of about 20 mm. is used. For the measurement of mica, 1 mm. to 2 mm. was found to give quite satisfactory results.

With a concave mirror at a distance of about 5 metres from the screen and a base of 1 mm. the deflection produced by inserting a thin piece of mica under the third leg magnified the thickness 10,000 times. For calibrating, a piece of mica of approximately the same thickness may be measured with apparatus of the most accurate class and then used as a standard.

The hole and slot arrangement is used for the support of the "twin" feet of the tilting table and a plane or slightly convex surface for the third leg.

These are conically pointed screws which should not bear on their points when it is important to replace the lever accurately in the same position. The third point should not be too sharp, but care must be taken lest its roundness produce an error. For the measurement of paper, &c., a small piece of plain glass, such as may be cut from a microscope slide, may be used to distribute the pressure. The lever may be made a foot long if used to measure over the surface of a large sheet which cannot be cut.

The arrangement would be particularly useful in comparing a number of articles of the same nominal size, such as bicycle balls, if it were very important to have greater equality in a set than could be tested with the ordinary micrometer.

The arrangement has been adapted to measure dilatation coefficients of rods of different materials. A rod of about

150 mm. in length has its ends made flat, and is then supported on a small foot. Its upper end carries a cap on which rests one leg of the lever. Two quartz rods similarly mounted are placed one on either side. The feet and caps of the rods are all similar. They have small cylindrical holes into which project the 90 deg. points of three screws about 12 mm. apart. These vertical screws at the bottom have their points accurately in a horizontal line. They are fixed in a horizontal brass bar, and any movement of this, due to temperature change, has no effect on the movement of the mirror, which is supported by the top of the three rods. The middle one is not quite parallel to the other two, its top being about 1 mm. out of the plane containing the other two rods. This arrangement gives a reading of about 75 mm. per degree for the difference in expansion between brass and quartz of 150 mm. length. The whole is supported from the bottom bar by two vertical rods from an overhead bracket. The rods are immersed in water in a separately supported tank.

Another use for the tilting table is for the measurement of the bending of cantilevers, &c. Three parallel rods project horizontally from the same block of metal, and each supports one of the legs of a tilting table at its free end. If the middle one have a weight added near its free end the deflection is greatly magnified. The instrument shown gave readings of 16.29, 16.29, 16.28 in. deflection for a weight of about 30 grammes using brass rods $\frac{1}{4}$ in. diameter and 200 mm. long, and a tilting table of 4 mm. "base," made of three needle points stuck in a piece of sheet brass.

This affords what may be a very valuable piece of apparatus as an adjunct to a chemical balance. In ordinary weighings the nearest centigramme could be read straight off, and the body then transferred to a balance and its weight readily found to 1/10 mgm. if desired.

The range of a single instrument could be made very large by extending the centre rod by a lighter one for smaller loads. Very large masses could be weighed by using appropriate cantilevers. By this means any weight can be readily determined to an order of accuracy of one part in 1,000 or so.

For calibrating the levers, when such is necessary, a specially designed lever of ratio of about 50 to 1 was shown, attention being particularly paid to obtaining the accurate length of the shorter arm. The longer arm is moved vertically by a micrometer.

In using apparatus of this description it is essential for con-

venience to reflect the deflected beam of light, by means of a plane vertical mirror, back again to a scale placed near the apparatus.

DISCUSSION.

Dr. S. RUSS asked what was the smallest thickness the optical lever could be used for. He had measured small thicknesses of mica by finding its transmissive power for ionisation.

The PRESIDENT remarked that it was interesting to see to what wide applications the optical lever could be put. The chief difficulty with high power optical levers, especially if the angular displacement was large, was with the shape of the points and the holes into which they fitted.

The AUTHOR, in reply, stated that the smallest thickness he had used the lever for was 3μ . For accurate work the hole, slot and plane mounting should be used, the middle leg resting on the plane.

XVII. *A Demonstration of the Working of the Anschütz Gyro Compass.* By G. K. B. ELPHINSTONE.

READ MARCH 10, 1911.

[ABSTRACT.]

THE instrument is really a compass, inasmuch as it points in a definite direction on any part of the earth's surface—that is to say, true North and South.

The apparatus consists of a rapidly rotating wheel enclosed in a casing which is so suspended that the centre of gravity of the suspended system is considerably below the point of suspension, and therefore acted upon by gravity like a pendulum.

With the axle of the gyro in any direction except parallel to the Meridian the gyrostatic action of the rapidly rotating wheel tends to maintain the plane of rotation fixed in space irrespective of the rotation of the earth, and, in consequence, a dipping down of one end of the axle and an upper inclination of the other end takes place.

The force of gravity exercises a couple tending to restore the axle to the horizontal position, and a precession results which turns the axle towards the Meridian, this precession being reversed should the axle once more reach the horizontal position after it has crossed the Meridian.

A natural tendency of a gyrostat suspended in a frame as described above, provided the method of suspension introduced very little friction, would be to swing from side to side of the Meridian for a very long period of time.

On this account a method of damping these swings without the introduction of friction is required. This is obtained by making use of the current of air set in motion by the rapidly revolving wheel. This current of air is led out through a horizontal tube at the lowest part of the gyro casing and tangential to its periphery; the air blast acts horizontally.

When the gyro axle is horizontal—that is to say, when precession in any one direction has ceased—the air blast is symmetrically situated as regards the vertical line through the suspended system, and on this account the air blast may be considered as divided into two blasts equal in speed and volume and distance from the centre line, and under such conditions there is no tendency for the re-action of the still air to cause any movement of the gyro casing other than a slight swinging of the suspended system in a plane coincident with

the plane of rotation of the gyro. If, however, the axis of the gyro is inclined from the horizontal position, which is the case whenever the gyro is precessing and therefore not coinciding with the Meridian, then the outlet for the air current is tilted to one side of the centre line, and the reaction caused thereby is in such a direction as to apply a turning moment about the vertical axis of the suspended system, and under the influence of this turning moment a precession of the gyro axle takes place vertically in such a direction as to bring the axis once more horizontal.

In considering the gyro at different points on the earth's surface it is clear that if the two ends of the gyro axle be equal in weight, and if the gyro hangs perfectly freely so that the axle is horizontal when the gyrostat is not running, that under these conditions it is only at the Equator that the axis of the gyro and the axis of the earth can be said to be strictly parallel to one another.

In such a position the axis of the gyro travels on the surface of a cylinder when the earth rotates.

At any other latitude than the Equator the axis of the gyro, being constrained to remain horizontal on the earth's surface, will travel round in space on the surface of a cone, and under these conditions a constant precession of the axle in space must take place, the value of this precession being proportional to $\omega \sin \lambda$, where ω is the angular velocity of the earth and λ is the angle of latitude.

The moment the axle begins to lag behind the Meridian a portion of $\omega \cos \lambda$ causes a tilt, which introduces the gravity couple, but simultaneously the damping couple tends to destroy this by making the axle horizontal. Hence there will be some position of lagging behind the Meridian, for which the damping just maintains the axle at that tilt at which the gravity couple can cause the necessary precession about the vertical. By the action of the earth's rotation the gravity couple is always tending to be increased by an amount which the damping is simultaneously tending to diminish, and thus the couple is kept constant to cause the constant precession $\omega \sin \lambda$.

It is, of course, clear that this precession is only constant for a given latitude. By placing a small weight on one end of the gyro spindle a couple could be introduced to suit any particular latitude, and yet the axle can remain horizontal; but any change of latitude from that for which the weight is adjusted will necessitate a small correction.

If the axle of the gyro is pointing North and South it is clear that any movement of the ship due East or West is merged in the movement due to the earth's rotation, and is negligible in amount.

A movement, however, due to steering a northerly course will produce a couple deflecting the gyro compass westwards, owing to the fact that the axle always tends to keep to its direction in space. Similarly, in the case of a southerly course, the compass is deflected eastwards.

DISCUSSION.

Dr. C. CHREE commented on the two corrections which had to be applied, one of which depended upon the component of the ship's velocity along the Meridian. He inquired as to the stress in the rotating wheel, and as to how long the material would stand that stress continually. He remarked upon the importance of accurate steering for gun fire, and saw in this direction an important sphere for the gyro compass. He wanted to know if pitching and rolling had any effect on the compass.

Mr. A. CAMPBELL asked what power the compass took.

Dr. A. RUSSELL asked how far Dr. Anschütz got in making a gyro compass with three degrees of freedom, so that it always pointed parallel to the earth's axis, and remarked that if another such gyro was made so as to keep parallel to the direction of gravity at Greenwich the two together would give both the latitude and longitude of a ship.

Mr. C. W. S. CRAWLEY asked how the gyro compass withstood gun fire.

Capt. L. CHETWYND, R.N., said he looked to the gyro compass as the way of helping them out of their greatest difficulties. It had come along just at the moment when the difficulties connected with the magnetic compass in modern warships had become almost insuperable. Replying to Mr. Crawley, he said the gyro compass had been very severely tried under gun fire, which had no effect whatever on it.

Mr. ELPHINSTONE, in reply, said that as the compass was suspended on gimbals pitching and rolling had no effect. The stress in the rotating wheel, which was made of special Krupp steel, was about 10 tons/in.² Every compass was tested by being run for 700 hours continuously. Several had been in use on and off for three years, and some had run 4,000 hours continuously without mishap. In reply to Mr. Campbell, he stated that the power taken was from 1 to 1.25 amperes in each phase at 110 volts. In reference to a gyro of three degrees of freedom, mentioned by Dr. Russell, he said that the constructional difficulties would be enormous. It would be necessary to keep the centre of gravity absolutely at the intersection of the axes about which the gyro frame rotated while the gyro was running. Gyros with three degrees of freedom had been tried in the German Navy, but given up in favour of the Anschütz, of which about 40 were in use.

XVIII. *Note on an Electrical Trevelyan Rocker.* By W. H. ECCLES, *A.R.C.S., D.Sc.*

RECEIVED FEBRUARY 16, 1911. READ MARCH 10, 1911.

ALL students of acoustics will remember the curious method of producing sounds that goes by the name of the Trevelyan Rocker experiment. A bar of brass or copper whose cross-section possesses two adjacent salient angles, is heated to some temperature below red heat and laid across a cylinder of lead, with the edges corresponding to the angles just mentioned resting on the lead. If circumstances are favourable, the apparatus yields a loud musical note which persists till the temperature of the copper bar has fallen considerably. The explanation of the experiment is this: When one copper edge touches the lead the heat passing from the copper to the lead causes the latter to expand locally; the expansion is so sudden that the edge is kicked away and the copper bar given a rotary motion which brings the other copper edge down on the lead. This edge now causes expansion and is in its turn repelled. The process may take place hundreds of times a second and usually with sufficient regularity to give a musical note. Lead is the best common metal to use because of its high coefficient of expansion with temperature and its low thermal conductivity, but other metals can be made to work.

It is an obvious elaboration of this experiment to pass a current of electricity from one metal to the other with a view to creating the necessary local heatings and expansions. The experiment was performed by Rollman * in 1850, and independently by Forbes † in 1859, and large masses can be made to rock by means of large currents at low voltages. A discussion of the whole matter by Tanner ‡ leads to the conclusion that all observers of the phenomenon agree that the current is never completely interrupted during the rocking action, one of the alternate contacts being "made" before the other is "broken." It is clear that a considerable current must be driven through the contact points if energy enough is to be supplied to keep a heavy rocker in full motion, but quite small currents can be used for vibrating small masses. Apple-

* Poggendorfs "Annalen," Vol. CV., p. 620.

† "Phil. Mag.," Vol. XVII., p. 358.

‡ "Electrical Review," Aug. 22, 1890.

yard * describes a large microphone consisting of a horizontal carbon rod laid across two other horizontal carbon rods fixed on a resonance box, which gives a loud sound with currents of about one ampere.

The following experiments were projected during a search for a method of impressing regular and sudden interruptions on a small current; the photographic records given below were obtained only recently. The sudden making or breaking of the current passing through the inductance of a vibratile circuit affords a very good method of generating feeble electrical oscillations if the discontinuities can be made with

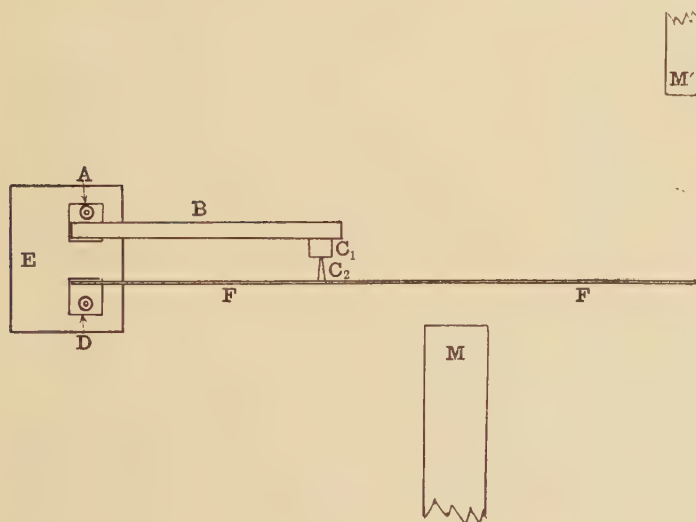


FIG. 1.

great and equal suddenness every time. The Trevelyan Rocker appeared likely to give sharp interruptions and was tried. It did not prove so successful for the purpose as other methods, and was therefore put aside. The present note describes the experiments for the sake of any intrinsic interest they may possess.

A good way of setting up the electrical rocker or vibrator was hit upon by adapting a simple piece of apparatus previously used by the writer in the study of microphonic contacts. This is shown diagrammatically in plan in Fig. 1. Here

* "Electrical Review," May 14, 1897.

E is an ebonite block carrying a stout brass bar, B, and a thin iron wire, F. These carry the pieces of conductor C_1 , C_2 , that form the contact. By means of the terminals A and D a current can be sent through the contact. The circuit usually included also a potential divider, a telephone, and a shunted fibre galvanometer. The apparatus is employed in the following manner. The permanent magnet M is pushed towards the iron wire, F, till the contact is just broken, the magnet M' being meanwhile at a considerable distance. Now M' in its turn is brought towards the other side of the iron wire till the contact is re-made—an event that produces a sound in the telephone. By arranging that M' is always a fair distance from the iron wire exceedingly delicate adjustments of the contact can be made. In short, M is a coarse adjustment and M' is a fine adjustment. At some stage of these adjustments the Trevelyan Rocker action starts. In most cases the best position of M' is quite well defined, which indicates that the range of proper adjustment is limited to within a fraction of a micron. As an example, on one occasion M was 3 cm. from the iron wire and M' was 10 cm. from the wire, while a motion of about 2 mm. of M' from its proper position would stop the vibrations.

The best of the common metals for getting the vibrations is lead; and it is well to arrange that C_1 is a plate and C_2 a point. With a suitable current and with occasional adjustment of the magnet M' the vibrations can be maintained for an hour at a time. Of course, there is a loud rattling noise in the telephone (the sound is scarcely ever musical when a rather long wire is used), and the loudness can be varied by altering the value of the current through the contact. The direction of the current is immaterial. In size the current must be kept below a certain small limit or the self-induction of the telephone gives rise to destructive sparking. The amount of mechanical motion of the wire F is so small as to be imperceptible to the eye even when assisted by a magnifying glass, and to be inaudible, however close the ear is held to the contact. There is no doubt about the existence of mechanical vibration, however, for in the course of a short time the lead point always becomes flattened by the hammering action at the contact. It is this flattening of the point that makes occasional adjustment of the magnet M' necessary if the apparatus operates for long. Harder metals than lead have been used, but none give such definite make and break or such

regularity as lead. A copper point against lead works very well, steel against steel works irregularly and rather weakly. Pairs of such substances as galena, pyrites, graphite and the

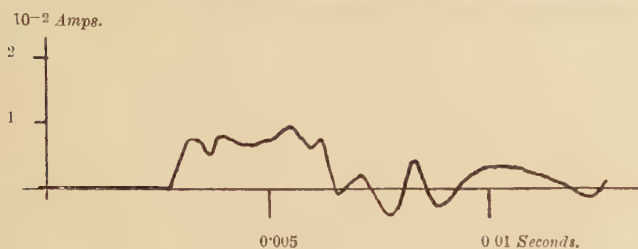


FIG. 2a.—LEAD AGAINST LEAD.

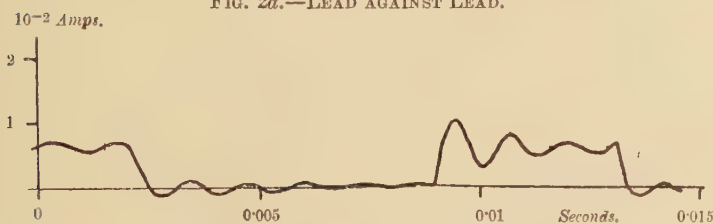


FIG. 2b.—LEAD AGAINST LEAD.

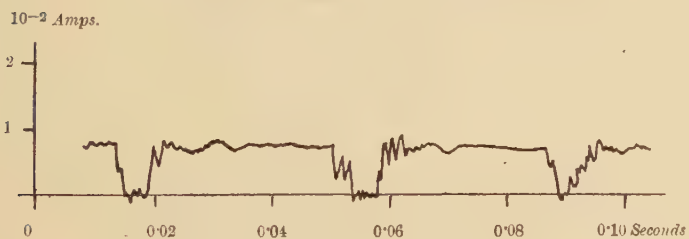


FIG. 2c.—STEEL AGAINST STEEL.

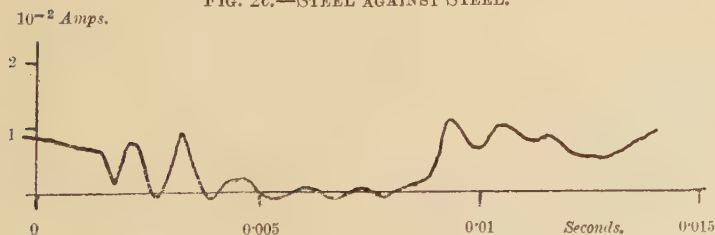


FIG. 2d.—STEEL AGAINST STEEL.

metals can be made to vibrate by careful adjustment. The above conclusions were reached by observations with the telephone. In Fig. 2 are given a few tracings of selected

photographic records obtained by aid of a fibre galvanometer and a falling plate camera. The fibre was rather too heavy and insufficiently damped for this particular purpose, and therefore allowance must be made for the fibre's own vibrations in interpreting the photographs. The two upper tracings are from lead-lead contacts, the lower ones from steel-steel contacts. The first of the two figures relating to lead shows that the interruptions are very sudden, but the second tracing indicates that great irregularities are possible. The interruptions at steel contacts are usually very irregular. The first steel tracing, which is drawn on a different scale from any of the others, shows three consecutive makes and breaks; the second figure is a typical make and break, and is extremely irregular. ¶ ¶

The interruptions produced by the Trevelyan Rocker action could in all cases be used for generating high-frequency electrical oscillations in the manner indicated above, but showed no practical advantage over directly driven interruptions. That is to say, the "make" and "break" of the current is not more sudden than occurs in vibrating contacts of the ordinary type of make and break apparatus.

XIX. *Some Notes on the Tilted Gold Leaf Electrometer, with Suggestions as to the Manipulation of Gold Leaf Suspensions, Suitable Insulators for Electrometer Work, &c.* By G. W. C. KAYE, D.Sc., B.A., *The National Physical Laboratory.*

READ MARCH 10, 1911.

THE tilted electrometer, designed by C. T. R. Wilson * in 1903, has the advantage of a small electrostatic capacity (a few centimetres as compared with about 80 for a Dolezalek electrometer), and offers at will not only a possible high sensitivity but a large range of sensitivities.

This Paper gives an account of a new form of the electrometer, in which the inconveniences of the early model (the extended use of which was never contemplated by Mr. Wilson) have, it is hoped, been removed.

An appendix to the Paper deals with the cutting and mounting of gold leaves, while in a second appendix some electrical insulators are discussed.

The Principle of the Tilted Electrometer.

The principle of the tilted electrometer may briefly be explained. An insulated gold leaf, L (Fig. 1), hanging from C is attracted out of the vertical by an inclined charged plate, P, in its proximity. An alteration in the potential of either, causes L to move. It can be shown that depending on

- (1) The tilt of the plate P,
- (2) The relative potential of P and L,
- (3) The proximity of P and L,

the equilibrium of the leaf may either be stable over the whole of its path, or unstable over part of it.

Fig. 2 illustrates the possibilities that may occur with the electrometer, the potential of the plate being kept constant. In curve I. the deflection of the leaf increases steadily with the potential on it, and throughout there is complete stability and only moderate sensitiveness. In curve III. there are, for a certain range of potentials, two positions of equilibrium for the leaf. The central dotted portion of the S is experimentally unrealisable, and indicative of instability.

By adjusting the conditions, curve II. can be obtained. Here, the deflection is single-valued throughout and although

* C. T. R. Wilson, "Proc. Camb. Phil. Soc.," XII., p. 135, 1903.

the sensitiveness is high over part of the path, there is complete stability.

The critical curve between stability and instability will have a vertical portion indicating infinite sensitiveness; and the tilted electrometer derives its sensitiveness by working as near as may be expedient or desirable to this critical state.*

It is evident that, except for moderate sensitiveness, leaf deflections will not be proportional to leaf potentials over any considerable range. Such proportionality cannot exist at high sensitiveness for any instrument which owes its sensitiveness, as this does, to its ability to work in a condition near instability.

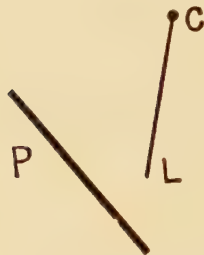


FIG. 1.

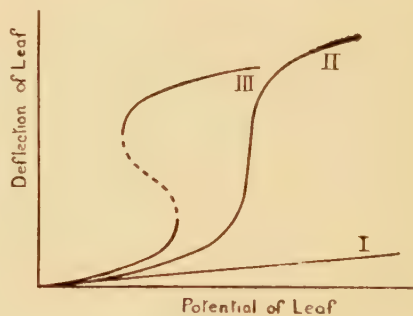


FIG. 2.

Description of the Electrometer.

Fig. 3 gives a general view of the electrometer which is shown in section to scale in Fig. 4. An earth-connected rectangular case, B (Fig. 4), contains within it the insulated plate, P, which can be screwed in or out by rotating the ebonite disc, D. P is connected through the terminal screw T to a source of steady potential (about 200 volts). E is an ebonite sleeve designed as an additional protection for the observer when adjusting D with T "alive."

The gold leaf, L, is supported by means of a wire, which passes through fused silica insulation in the case to the make and break cup, C. A is an earthed tube for shielding purposes. The leaf is observed through the small windows in the case, by a microscope which is provided with a scale in the eye-piece,

* The sensitiveness of "String Electrometers" is similarly derived. See Laby, "Proc. Camb. Phil. Soc.," XV., p. 106, 1909.

and has the necessary adjustments for following and focussing the leaf. The whole is mounted on a substantial rigid base which can be tilted by the screw, S (Fig. 3), and carries a binding screw for earthing the case.

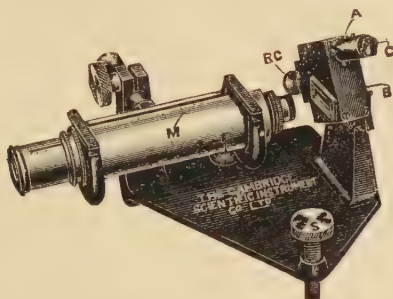


FIG. 3.

The Gold Leaf System and Connections.

The electrical connections are shown in Fig. 5 ; most of the features are referred to elsewhere.

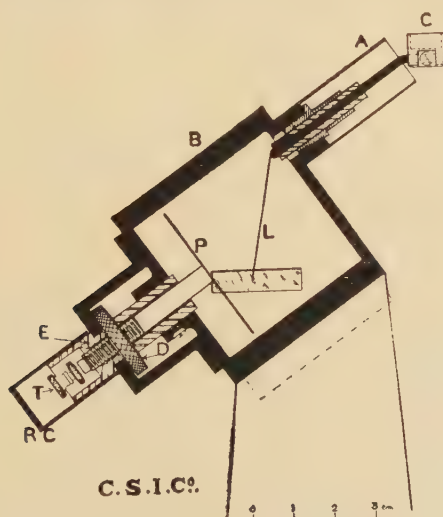


FIG. 4.

Mercury should not be used in the cup, C ; it exhibits too much electrification by "splashing," which is frequently evidenced by slight charging up of the gold leaf when the earth connection to the cup is broken. A strong solution of calcium

chloride is convenient and free from this defect ; its use entails cleaning out the brass cup every few months. If for some reason mercury is preferred, an iron cup should replace the brass one.

The zero-position of the leaf is obtained by earthing the key, K, and lowering it (*e.g.*, by means of a string over a pulley) into the cup, C. When K is raised the leaf is insulated.

The key, K, may also to be connected to a variable known potential which may be used to calibrate the scale of the electrometer. If the sensitiveness is high, only a fraction of a volt will be necessary for the testing potential.

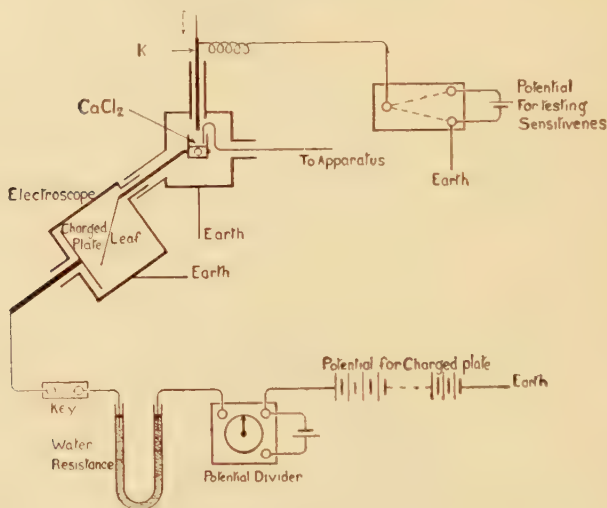


FIG. 5.

A check on the constancy of the potential of the charged plate is provided by the constancy of the zero of the leaf when earthed.

The Gold Leaf.

The length of leaf should lie between 2.9 and 3.4 cms.* Its width may conveniently be 1 mm or less. Methods of cutting gold leaves are discussed in the appendix.

When a new leaf is required, the cup, C, is removed, and the instrument canted over on its side. In this position the gold leaf

* This, of course, applies only to the present form of the instrument.

support and its mounting are vertical and can be withdrawn or inserted without endangering the attached leaf.

The plate, P, is screwed in or out until the end of the leaf *just* clears it. The electrometer is then canted back into the normal position, and the cup, C, is restored.

These relative positions of the gold leaf and charged plate make for the maximum sensitiveness consistent with the safety of the leaf. Should the end of the leaf come in contact with the plate or sides of the case and persist in adhering, the trouble (which is probably due to grease) can be eradicated by taking the electrometer apart, removing the glass windows and the insulation, and boiling the metal case in distilled water with a little soda added.

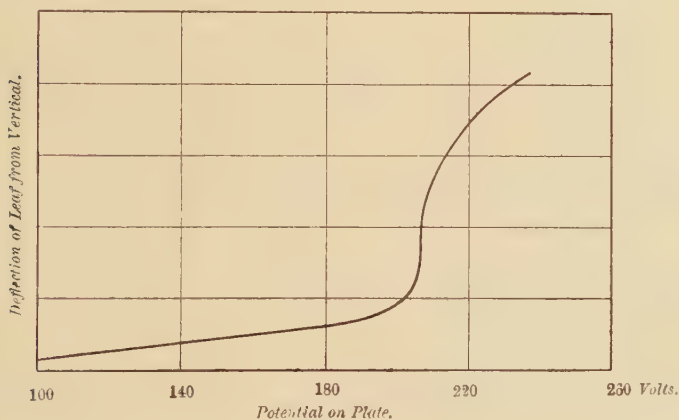


FIG. 6.

The windows are of parallel glass and will ordinarily give no trouble owing to electrification, but windows of conducting glass * can be substituted if desired.

The High Potential Cells.

Good reliable cells, which will maintain the potential of the charged plate, P, of the electrometer constant for some time are a *sine qua non* for delicate measurements. The constancy of the zero is controlled of course by the steadiness of the potential. Together with all the connections, &c., the cells

* Conducting glass owes its origin to Mr. C. E. S. Phillips, and is procurable from Mr. A. C. Cossar, Farrington-road. Its specific resistance is about 10^5 cm. ohms., *i.e.*, it conducts about 500 times better than ordinary German soda lime glass.

should be well insulated, and a water-resistance should always be in the circuit between the cells and the electrometer as a precaution against accidental short-circuiting. (See Fig. 5.)

A potential divider which will give a volt or so, and act as a fine adjustment on the potential of P may form part of the circuit; it may, of course, be unnecessary.

Some Typical Curves.

Fig. 6 reproduces a typical curve * showing the relation between the potential on the charged plate and the deflection of the leaf from the vertical, the leaf being kept earthed and

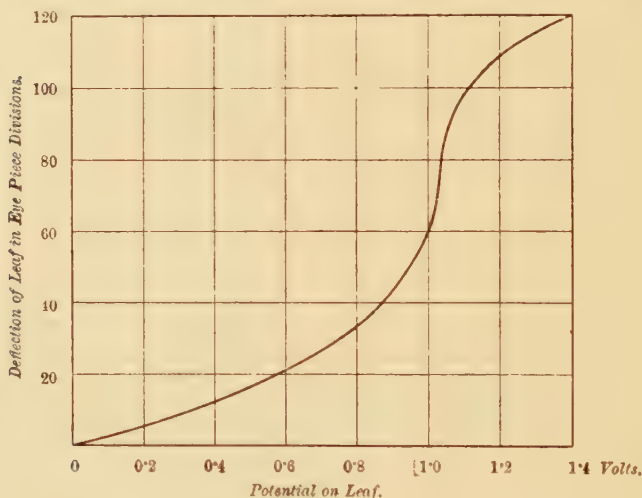


FIG. 7.

in metallic connection with the case. It will be seen that for the particular tilt employed (about 30°), the sensitive region is in the neighbourhood of 207 volts.

Fig. 7 shows a typical relation between the potential on the leaf, and its deflection from the zero. The plate is at 207 volts and the tilt about 30° . It will be seen that in this case the sensitiveness over the central region of the curve is at the rate of some 300 divisions (about $5\frac{1}{2}$ mms.) per volt.†

* Taken from Mr. Wilson's Paper above.

† It has become customary to give the sensitiveness of electroscopes in eye-piece divisions per volt irrespective of the magnification and the size of the divisions. Sensitiveness should, of course, be expressed as actual distance moved by the leaf per volt.

Some Adjustments.

1. With an average length of leaf and about 200 volts on the plate the sensitive region will be somewhere near the middle of the window when the base of the electrometer is horizontal.

2. For any particular tilt of the electrometer there is a plate potential which will yield a maximum sensitiveness. The less the tilt (or the angle of the plate to the vertical), the greater is the plate potential required and the greater the maximum sensitiveness, until the critical tilt is reached beyond which comes instability over some region; the extent of this unstable region increases as the tilt diminishes.

3. The greater the sensitiveness the more limited is the range of that sensitiveness and the more susceptible is the instrument to a change in the conditions.

4. To secure the same sensitiveness from a short leaf as from a long requires a higher plate potential and a smaller angle of tilt. A variation in the plate potential of from 160-240 volts may thus be required.

5. As a fine adjustment on the conditions, either the potential, the position of the charged plate, or the tilt may be altered—whichever is most convenient.

I should like to acknowledge my indebtedness to Mr. R. S. Whipple, of the Cambridge Scientific Instrument Co., to whose experience and ability the new design of the electrometer owes a good deal. Mr. C. T. R. Wilson's kindly interest and stimulating suggestions are gratefully recorded.

APPENDIX I.

On the Cutting and Mounting of Gold Leaves.

Most observers have their own way of avoiding the difficulties which attend the cutting of leaves by the novice. There is no point in cutting them narrower than say, $\frac{1}{2}$ mm. for electroscope work; 1 mm. or 2 mms. is very convenient. Theoretically, a narrow leaf enjoys no advantage over a broad one, but in practice it is found that a narrow leaf is better, being more flexible at the "hinge"* and less perturbed by convection currents.

One good way of cutting gold leaves is to take a book of

* For this reason gold leaf is to be preferred to aluminium leaf, which cannot be obtained quite so thin. Weight for weight there is nothing to choose between the two metals in the thinnest leaves in which they are at present procurable.

leaves, isolate one of the metal sheets between the two adjoining grease-free paper leaves, lay them on a piece of cardboard, and cutting with a steady slow stroke through all three sheets by means of a well set razor, separate off a strip of the desired width.

One other method * of cutting leaves may be mentioned. A pellet of paraffin wax is heated on one side of a clean glass plate which thus becomes coated with a film of wax. While the wax is still liquid, the plate is laid film downwards on an uncreased sheet of gold leaf which is then lifted away with the plate. A second pellet of wax is put on the leaf and a further warming of the plate coats the leaf with wax. When cold, the leaf can be cut into strips of any shape and width by a penknife and ruler.

On warming the plate, a strip can be lifted off on the point of a pin; the wax resets on the strip and facilitates its manipulation. The strip is immersed in xylol to remove the wax; it is withdrawn from the liquid on a strip of grease-free paper detached from a book of leaves. The leaf is then freed from the xylol by immersion in absolute alcohol, from which it is withdrawn and allowed to dry on grease-free paper. It dries perfectly flat and is ready for use in a few minutes. Very long, narrow and straight-edged leaves can be prepared in this way.

If desired, the free end of the leaf can, for greater sensitiveness, be expanded into a sort of fantail, but this can scarcely be said to be essential and, unless the wax method of cutting is adopted, requires very considerable manipulative skill. In cutting down a leaf to its length, about 2 mms. should be allowed for the "hinge." When a suitable leaf is ready for mounting, a touch of soft red wax, gum or natural hair grease (which is the resort of bookbinders and other manipulators of gold leaf) is applied to the chisel-ended extremity of the support; this will be sufficient to pick up and affix the leaf. If a point about 2 mms. from one end is selected for the hinge, the surplus length can be pressed down on the wire to improve the metallic contact. The mounted leaf is then lowered into position in the case.

APPENDIX II.

Electrical Insulators.

Of the available insulators, ebonite, sulphur, amber, sealing wax and fused silica are at present the only ones at all suitable

* Beatty, "Phil. Mag." XIV., p. 605, 1907.

for electrometer work. With all of these, care should be taken to avoid fingering—grease is fatal to insulation: the remedy is good soap and water. In testing insulation it should be remembered that a delicate electroscope may indicate signs of surface electrification for some hours after new insulation has been put in.

Good *ebonite* is difficult to obtain nowadays; it seems to be regarded by most rubber manufacturers as a convenient means of using up rubber refuse unfit for anything else. Some of its defects are occasionally due to the materials used in polishing. Modern ebonite ages with some rapidity in sunlight, and on damp days may, owing to the film of sulphuric acid which forms on its surface, almost play the rôle of a conductor. In a room which gets much sunshine, most modern ebonite usually turns a sort of dirty yellow colour in a few weeks, though some of the ebonite made 10 or 20 years ago will exhibit no signs of deterioration. French chalk can advantageously be applied to restore deteriorated ebonite surfaces. Notwithstanding its defects, ebonite which has had its surface recently renewed is an excellent insulator. Ebonite offers, of course, the great advantage of being easily workable.

Sulphur is convenient in that it can be cast to shape. Needless to say, in this operation the vessels (glass or porcelain) and sulphur used should be clean, and the temperature should be raised no higher than will *just* produce liquefaction. In this limpid condition, it can, for example, be poured into clean warmed glass* tubing if sulphur plugs are required. The tubing can be readily slipped off later by slightly warming the outside. For some hours after solidification sulphur can be turned to size or pared to shape with great ease. There are no better insulators than sulphur, but, after a few months, especially in a room which gets much sunshine, its insulating qualities generally fall off considerably.

Amber is an excellent insulator and is almost always reliable. It can, of course, be obtained in the form of pipe stems which can be mounted in position with sealing wax. The Amberite and Ambroid companies supply amber pressed to convenient shapes and sizes. Amber has the disadvantages of being somewhat brittle and rather expensive.

Sealing Wax is particularly useful in combining the qualities of an insulator and an air-tight cement. The insulating pro-

* Not metal, unless lined, say, with paper.

perties depend very much on the quality of the wax. One of the most reliable is "Bank of England." The usual care must be taken to avoid indiscriminate fingering. The insulating ability of the wax will be impaired if in its manipulation it is allowed to catch fire and carbonise, or if a luminous flame is used. As shellac is hygroscopic, sealing wax as an insulator is somewhat susceptible to damp weather.

Good *Fused Silica* yields place to none in its insulating qualities. Its specific resistance has been determined at the National Physical Laboratory to be greater than 2×10^{14} ohms cms. at 16°C . Fused silica is practically independent of atmospheric humidity, and in the form of rod or tubing is particularly convenient as an insulating material. It is the only high class insulator which is unimpaired by moderate heat; it is however spoilt by very high temperatures.

* Fused silica is now very cheap, but unfortunately the modern furnace methods of production cannot be relied upon to yield a product which possesses the insulating properties of the more expensive silica made by the oxyhydrogen flame. This remark applies alike to the clear transparent variety and the air-streaked satin-like kind. In some way the furnace silica seems to be contaminated, possibly by the carbon from the electric furnace. Silica intended for insulation purposes should, of course, be alkali free.

ABSTRACT.

F A new model of the C. T. R. Wilson tilted electrometer is described and its mode of use and method of adjustment are gone into. The instrument is provided with an adjustable charged plate, fused silica insulation, a microscope mounted on the same stand as the electroscope case, and has a number of other conveniences. The depth of the case is such as to permit the use of a high power objective.

The electrometer has a small electrostatic capacity, and owes its high sensitiveness to its ability to work in a condition near instability.

An appendix deals with the manipulation of gold leaf suspensions, and reviews the various insulators which are at present available for refined electrometer work.

XX. *The Roots of the Neumann and Bessel Functions.* By
JOHN R. AIREY, *M.A., B.Sc.; late Scholar of St. John's
College, Cambridge.*

FIRST RECEIVED DECEMBER 29, 1910. RECEIVED IN ABRIDGED FORM
FEBRUARY 10, 1911.

THE first 40 values of x for which the Bessel function of the zeroth order, $J_0(x)$ vanishes, have been calculated by Willson and Peirce,* whilst a general expression for the roots of $J_p(x)=0$ has been given by MacMahon.† The second solution of the Bessel differential equation—Neumann's solution—is often referred to as a Neumann function, and is usually denoted by $Y_p(x)$. Only the first four roots of $Y_0(x)=0$ and the first three roots of $Y_1(x)=0$ appear to have been given. These were found by graphic interpolation from Smith's tables‡ by Kalähne §:—

Roots of $Y_0(x)=0$.	Roots of $Y_1(x)=0$.
0·826(0)	2·11(8)
3·88(5)	5·35(5)
7·01(3)	8·52(1)
10·14(9)	—

The general expression for the calculation of the roots of $Y_p(x)$ and the roots of $\mu J_p(x) + Y_p(x)$, where μ is any real number, positive or negative, can be found by adopting the method employed in determining the roots of $J_p(x)$.

$$\text{Roots of } \mu J_p(x) + Y_p(x) = 0.$$

The equation may be written in the form

$$\mu J_p(x) + (\log 2 - \gamma) J_p(x) + \frac{\pi}{2} N_p(x) = 0. \quad . \quad . \quad (1)$$

The function ¶ $N_p(x)$ is the same as $-Y_p(x)$ [Schafheitlin] $K_p(x)$ [Graf. u. Gubler] and the Neumann function of Nielsen, $Y_p(x)$.

* "Bulletin" of the American Mathematical Society, Vol. III., 1896-7.

† "Annals of Mathematics," Vol. IX., 1895.

‡ B. A. Smith, "Messenger of Mathematics," 26, 1897.

§ Kalähne, "Zeitschrift für Mathematik u. Physik," 54, 1907.

¶ Jahnke u. Emde, Funktionentafeln, 1909.

Writing α for $\mu + \log 2 - \gamma$, and substituting the semi-convergent series for the functions, equation (1) becomes

$$\left[\alpha P_p + \frac{\pi}{2} Q_p \right] \cos \left(x - \frac{2p+1}{4} \pi \right) + \left[\frac{\pi}{2} P_p - \alpha Q_p \right] \sin \left(x - \frac{2p+1}{4} \pi \right) = 0. \quad (2)$$

Case (i.), when α is equal to or less than $\frac{\pi}{2}$.

Divide by $\frac{\pi}{2}$, write b for $\frac{2\alpha}{\pi}$, and put

$$bP_p + Q_p = R \sin \theta_p,$$

$$P_p - bQ_p = R \cos \theta_p.$$

Then

$$\tan \theta_p = (bP_p + Q_p) / (P_p - bQ_p). \quad (3)$$

and (2) becomes $\sin \left(x - \frac{2p+1}{4} \pi + \theta_p \right) = 0$.

$$x = \frac{2p+1}{4} \pi + \theta_p = n\pi. \quad (n=0, 1, 2 \dots)$$

$$x = (4n+2p+1) \frac{\pi}{4} - \theta_p. \quad (4)$$

The expression for θ_p in powers of x is found by first calculating the value of $\tan \theta_p$ from (3) and applying Gregory's series to determine the angle from its tangent.

Writing m for $4p^2$ and y for $\frac{1}{8x}$, we get

$$\tan \theta_p = b + (1+b^2)(m-1)y + b(1+b^2)(m-1)^2 \cdot y^2 + \dots,$$

$$\text{and } \theta_p = \arctan b + (m-1)y + \frac{4}{3}(m-1)(m-25)y^3 + \dots \quad (5)$$

The coefficients of the even powers of y and all terms containing b in the coefficients of the odd powers of y vanish. Equation (4) takes the form

$$x = (4n+2p+1) \frac{\pi}{4} - \arctan b - \frac{m-1}{8x} - \frac{4}{3} \frac{(m-1)(m-25)}{(8x)^3} \dots$$

or, writing β for $(4n+2p+1) \frac{\pi}{4} - \arctan b$, and applying Lagrange's theorem,

$$x = \beta - \frac{m-1}{8\beta} - \frac{4(m-1)(7m-31)}{3(8\beta)^3} - \frac{32(m-1)(83m^2-982m+3779)}{15(8\beta)^5}, \dots \quad (6)$$

where $m=4p^2$, $b=\frac{2a}{\pi}=\frac{2(\mu+\log 2-\gamma)}{\pi}$.

Case (ii.) When a is equal to or greater than $\frac{\pi}{2}$, it is readily shown, as above, that x can be expressed in the same form as (6), if

$$b=\frac{\pi}{2a}=2(\mu+\log 2-\gamma) \quad \text{and} \quad \beta=(4n+2p+3)\frac{\pi}{4}+\text{arc tan } b.$$

These results can be tested when p is half an odd integer.

Roots of $Y_p(x)=0$.

The expression for the roots of this equation is

$$x=\beta-\frac{m-1}{8\beta}-\frac{4(m-1)(7m-31)}{3(8\beta)^3} \dots$$

where $\beta=(4n+2p+1)\frac{\pi}{4}-\text{arc tan } b$
 $= (4n+2p+1)\frac{\pi}{4}-0.073671.$

The following tables of the roots of $Y_0(x)$, $Y_1(x)$ and $Y_2(x)$ have been calculated from this formula, except in the case of some of the earlier roots. These were found by calculating the values of the Neumann functions to seven places of decimals in the neighbourhood of the roots from the expressions

$$Y_0(x)=J_0(x) \cdot \log_e x + \left(\frac{x}{2}\right)^2 - \frac{3}{8}\left(\frac{x}{2}\right)^4 + \frac{11}{216}\left(\frac{x}{2}\right)^6 \dots \&c.,$$

and then graphically representing these results.

TABLE of First Forty Roots of $Y_0(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	0.82601	21	63.54555
2	3.88456	22	66.68705
3	7.01256	23	69.82856
4	10.14876	24	72.97007
5	13.28748	25	76.11159
6	16.42729	26	79.25312
7	19.56766	27	82.39465
8	22.70837	28	85.53619
9	25.84930	29	88.67773
10	28.99037	30	91.81928
11	32.13154	31	94.96082
12	35.27278	32	98.10237
13	38.41409	33	101.24393
14	41.55544	34	104.38548
15	44.69682	35	107.52704
16	47.83823	36	110.66860
17	50.97966	37	113.81016
18	54.12111	38	116.95172
19	57.26258	39	120.09329
20	60.40406	40	123.23486

TABLE of Roots of $Y_1(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	2.11827	21	65.10862
2	5.35509	22	68.25048
3	8.52196	23	71.39231
4	11.67528	24	74.53412
5	14.82365	25	77.67592
6	17.96965	26	80.81770
7	21.11434	27	83.95947
8	24.25823	28	87.10123
9	27.40159	29	90.24296
10	30.54459	30	93.38470
11	33.68732	31	96.52642
12	36.82986	32	99.66813
13	39.97226	33	102.80984
14	43.11453	34	105.95154
15	46.25671	35	109.09324
16	49.39882	36	112.23493
17	52.54087	37	115.37661
18	55.68287	38	118.51829
19	58.82482	39	121.65996
20	61.96673	40	124.80163

TABLE of Roots of $Y_2(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	3.29690	21	66.65705
2	6.71703	22	69.79990
3	9.94840	23	72.94265
4	13.13551	24	76.08531
5	16.30478	25	79.22788
6	19.46500	26	82.37037
7	22.62002	27	85.51280
8	25.77173	28	88.65517
9	28.92124	29	91.79749
10	32.06920	30	94.93976
11	35.21601	31	98.08199
12	38.36197	32	101.22417
13	41.50726	33	104.36632
14	44.65204	34	107.50844
15	47.79639	35	110.65053
16	50.94040	36	113.79259
17	54.08414	37	116.93462
18	57.22763	38	120.07664
19	60.37093	39	123.21863
20	63.51406	40	126.36060

¹¹ Roots of $N_p(x)$ [Nielsen's $Y_p(x)$, &c.].

By making $\mu = \gamma - \log 2$, $a=0$ and $b=0$, equation $\mu J_p(x) + Y_p(x) = 0$ becomes $N_p(x) = 0$.

The expression for the roots of this function is found from (9) and agrees with that given by Prof. MacMahon, viz.,

$$x = \beta - \frac{m-1}{8\beta} - \frac{4(m-1)(7m-31)}{3(8\beta)^3} - \dots,$$

where $\beta = (4n+2p+1)\frac{\pi}{4} = (4n+2p-1)\frac{\pi}{4} + \frac{\pi}{2}.$

Roots of $N_0(x)$ and $N_1(x)$.

The first two roots of $N_0(x)$ and the first root of $N_1(x)$ were found by interpolation from values of these functions in the neighbourhood of the roots. These values were calculated from the expressions

$$N_0(x) = \frac{2}{\pi} \left[\left(\frac{x}{2}\right)^2 - \left(1 + \frac{1}{2}\right) \left(\frac{x}{2}\right)^4 + \frac{(2!)^2 + (1 + \frac{1}{2} + \frac{1}{3})}{(3!)^2} \left(\frac{x}{2}\right)^6 - \dots \right. \\ \left. - (\log 2 - \gamma - \log_e x) J_0(x) \right].$$

$$N_1(x) = \frac{2}{\pi} \left[-\frac{x}{2} + \frac{3}{4} \left(\frac{x}{2}\right)^3 - \frac{11}{72} \left(\frac{x}{2}\right)^5 + \dots \right. \\ \left. - (\log 2 - \gamma - \log_e x) J_1(x) - \frac{J_0(x)}{x} \right].$$

No. of root.	$N_0(x)$.	$N_1(x)$.
1	0.89358	2.19685
2	3.95769	5.42968
3	7.08605	8.59601
4	10.22234	11.74915
5	13.36110	14.89744
6	16.50092	18.04340
7	19.64131	21.18807
8	22.78203	24.33194
9	25.92296	27.47529
10	29.06403	30.61829

Roots of $\mu J_p(x) + Y_p(x) = 0$.

The first two or three roots of the equation, when $\mu=1$, 2, 3 . . . and $p=0$ and $p=1$, have been found graphically by Kalähne from tables of the Bessel and Neumann functions.

Roots of $J_0(x) + Y_0(x) = 0$. 0.356(5), 3.34(4)

Roots of $J_1(x) + Y_1(x) = 0$. 1.52(2), 4.80(2).

TABLE of First Ten Roots of $J_0(x) + Y_0(x) = 0$.

No. of root.	Root.	No. of root.	Root.
1	0.35617	6	15.88353
2	3.34532	7	19.02383
3	6.46999	8	22.16449
4	9.60543	9	25.30539
5	12.74386	10	28.44644

TABLE of First Ten Roots of $J_1(x) + Y_1(x) = 0$.

No. of root.	Root.	No. of root.	Root.
1	1.52101	6	17.42498
2	4.80339	7	20.56985
3	7.97498	8	23.71385
4	11.12971	9	26.85729
5	14.27867	10	30.00035

Roots of $J_0(x) - Y_0(x) = 0$, &c.

No. of root.	$J_0 - Y_0$.	$J_0 - 2Y_0$.	$10J_0 + Y_0$.	$10J_0 - Y_0$.
1	1.37616	1.11707	2.55629	2.25096
2	4.46696	4.19582	5.67354	5.36311
3	7.59752	7.32529	8.80753	8.49639
4	10.73440	10.46187	11.94545	11.63407
5	13.87338	13.60073	15.08488	14.77345
6	17.01333	16.74062	18.22505	17.91352
7	20.15378	19.88103	21.36564	21.05408
8	23.29454	23.02177	24.50649	24.19490
9	26.43549	26.16271	27.64750	27.33590
10	29.57658	29.30380	30.78864	30.47702

XXI. *The Vibrations of Circular Plates and their Relation to Bessel Functions.* By JOHN R. AIREY, M.A., B.Sc., late Scholar of St. John's College, Cambridge.

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THE vibrations of circular plates were first investigated by Poisson* in a celebrated memoir read before the French Academy of Sciences in 1829. Three cases were considered: (a) when the circumference was fixed; (b) when the plate was "supported"; (c) when the plate was free. The ratios of the radii of the nodal circles to the radius of the plate were calculated when the vibrating plate had no nodal diameter and one or two nodal circles. Kirchhoff† extended Poisson's results for the free plate by calculating six ratios of the radii when the plate vibrated with one, two or three nodal diameters, whilst Schulze‡ found eight more values of the ratios for a plate with fixed circumference.

The calculation of these ratios required the determination of the roots of equations involving Bessel functions with real and imaginary arguments. These appear to have been found by a "trial and error" method or by interpolation from tables of these functions.

The object of the present Paper is to give a general method of solving these equations—viz., equation (4A) for a circular plate with fixed circumference (Table I.), and equation (9A) for a free circular plate (Tables II. and III.). From the roots so calculated, the radii of the nodal circles and the times of vibration in any given mode are readily found.

(A) *Vibrations of a Circular Plate with Fixed Circumference.*

The displacement of a point on the plate from its position of equilibrium is given by

$$w = A \cos(p\theta) \{J_p(\kappa r) + \lambda J_p(i\kappa r)\} \cos(qt - \epsilon). \quad (1)$$

The boundary conditions in this case are $w=0$ and $\frac{dw}{dr}=0$ when $r=a$, or, if the radius of the plate is equal to unity, when $r=1$.

* "Mémoires de l'Académie royale des Sciences de l'Institut de France," tome VIII., 1829.

† Kirchhoff, "Pogg. Annalen," 1850. Strehlke, "Pogg. Annalen," 1855.

‡ Schulze, "Ann. der Physik," XXIV., 1907.

Hence (1) gives

$$J_p(\kappa a) + \lambda J_p(i\kappa a) = 0, \quad . \quad . \quad . \quad . \quad (2)$$

$$J_p'(\kappa a) + \lambda J_p'(i\kappa a) = 0. \quad . \quad . \quad . \quad . \quad (3)$$

Eliminating λ and writing x for κa , we get

$$\frac{J_p'(x)}{J_p(x)} = \frac{J_p'(ix)}{J_p(ix)} \quad . \quad . \quad . \quad . \quad (4)$$

or
$$\frac{J_{p+1}(x)}{J_p(x)} + \frac{I_{p+1}(x)}{I_p(x)} = 0. \quad . \quad . \quad . \quad . \quad (4A)$$

This becomes, after the substitution of the semi-convergent series for $J_p(x)$, &c.,

$$\tan \left(x - \frac{2p+1}{4} \pi \right) = \frac{I_{p+1} \cdot P_p + I_p \cdot Q_{p+1}}{I_{p+1} \cdot Q_p - I_p \cdot P_{p+1}}. \quad . \quad . \quad . \quad . \quad (5)$$

or
$$\tan \left(x - \frac{p\pi}{2} - n\pi \right) = \frac{I_p(P_{p+1} - Q_{p+1}) - I_{p+1}(P_p + Q_p)}{I_p(P_{p+1} + Q_{p+1}) + I_{p+1}(P_p - Q_p)} \quad (5A)$$

$$= -ay - 8ay^2 - \frac{a}{3}(m^2 + 2m + 93)y^3 - 8a^3y^4$$

$$- \frac{2a}{15}(m^4 + 6m^3 + 744m^2 - 10726m + 56055)y^5 \dots$$

where $a = 4p^2 - 1$, $m = 4p^2$ and $y = \frac{1}{8x}$.

Then, by Gregory's series,

$$\begin{aligned} x &= n\pi + \frac{p\pi}{2} - a \{ y + 8y^2 + \frac{4}{3}(m+23)y^3 \\ &\quad + \frac{1}{15}(96m^2 - 18624m + 110688)y^5 \dots \} \\ &= \beta - \frac{a}{8x} - \frac{8a}{(8x)^2} - \frac{4a(m+23)}{3(8x)^3} \\ &\quad - \frac{a}{15} \cdot \frac{(96m^2 - 18624m + 110688)}{(8x)^5} \dots \quad (6) \end{aligned}$$

By Lagrange's theorem, if

$$x = \beta + \frac{p}{x} + \frac{q}{x^2} + \frac{r}{x^3} + \frac{s}{x^4} + \frac{t}{x^5} \dots,$$

$$x = \beta + \frac{p}{\beta} + \frac{q}{\beta^2} + \frac{r - p^2}{\beta^3} + \frac{s - 3pq}{\beta^4} + \frac{t - 2q^2 - 4pr + 2p^3}{\beta^5} \dots$$

Hence

$$x = \beta - (m-1) \left[\frac{1}{8\beta} + \frac{8}{(8\beta)^2} + \frac{4(7m+17)}{3(8\beta)^3} + \frac{192(m-1)}{(8\beta)^4} + \frac{32(83m^2+218m+2579)}{15(8\beta)^5} \dots \right], \quad (7)$$

where

$$\beta = \left(\frac{2n+p}{2} \right) \pi.$$

Two roots of equation (4A) were given by Poisson when $p=0$, viz., 3.196 and 6.292, whilst Lord Rayleigh gave the values 3.20 and 6.3. For other values of p , Schulze found the following roots: When $p=1$, 4.612, 7.80, 10.95; and when $p=2$, 5.904 and 9.40.

The roots of equation (4) have been calculated from the expression given in (7), when $p=0, 1, 2$ and 3, some of the earlier roots by interpolation.

TABLE I.

$$\text{Roots of } \frac{J_{p+1}(x)}{J_p(x)} + \frac{I_{p+1}(x)}{I_p(x)} = 0.$$

No. of root.	$p=0.$	$p=1.$	$p=2.$	$p=3.$
1	3.1955	4.611	5.906	7.144
2	6.3064	7.799	9.197	10.536
3	9.4395	10.958	12.402	13.795
4	12.5771	14.109	15.579	17.005
5	15.7164	17.256	18.745	20.192
6	18.8565	20.401	21.901	23.366
7	21.9971	23.545	25.055	26.532
8	25.1379	26.689	28.205	29.693
9	28.2790	29.832	31.354	32.849
10	31.4200	32.975	34.502	36.003

By substituting one of these values in (2), the value of λ corresponding to this root can be found—*e.g.*, when the plate is vibrating with one nodal diameter and three nodal circles, $\kappa a = x = 10.958$, and

$$\lambda = - \frac{J_1(10.958)}{J_1(10.958i)}$$

or $i\lambda = 0.0000255 \dots$

Equation (2) becomes

$$J_1(\kappa r) + 0.0000255 I_1(\kappa r) = 0.$$

The roots of this equation are 3.8312, 7.0024 and 10.958.

Therefore, the ratios of the radii of the nodal circles to that of the plate are

$$\frac{3.8312}{10.958} = 0.3496 \text{ and } \frac{7.0024}{10.958} = 0.6390,$$

Schulze ("Ann. der Physik.," 1907) gives the values
0.350 and 0.640.

The expression for the frequency of vibration of the plate is

$$N = \frac{ax^2}{\sqrt{1-\mu^2}}.$$

where a is constant for the same plate (Lord Rayleigh, "Theory of Sound," Vol. I., § 217), μ is Poisson's ratio, and x is a root of equation (4). Since x is independent of μ , N is only affected by a change in the assumed value of μ through the factor

$$\frac{1}{\sqrt{1-\mu^2}}.$$

(B) *Vibrations of a Free Circular Plate.*

The boundary conditions require

$$-\lambda = \frac{p^2(\mu-1)\{xJ_p'(x) - J_p(x)\} - x^3J_p'(x)}{p^2(\mu-1)\{ixJ_p'(ix) - J_p(ix)\} + ix^3J_p'(ix)} \quad (8)$$

$$\text{and} \quad -\lambda = \frac{(\mu-1)\{xJ_p'(x) - p^2J_p(x)\} - x^2J_p'(x)}{(\mu-1)\{ixJ_p'(ix) - p^2J_p(ix)\} + x^2J_p'(ix)} \quad (9)$$

where x is written for κa .

Eliminating λ , the expressions on the right of (8) and (9) are equal. (9A)

(i.) When $p=0$, *i.e.*, when there is no nodal diameter, equation (9A) becomes

$$2(1-\mu) + ix \frac{J_0(ix)}{J_0'(ix)} + x \frac{J_0(x)}{J_0'(x)} = 0. \quad (10)$$

or, using Poisson's value for μ ($\mu = \frac{1}{4}$)

$$\frac{J_0(x)}{J_1(x)} = \frac{3}{2x} - \frac{I_0(x)}{I_1(x)}. \quad (10A)$$

Substituting the semi-convergent series for $J_0(x)$, $J_1(x)$, &c., we get

$$\frac{P_0 \cdot \cos\left(x - \frac{\pi}{4}\right) - Q_0 \sin\left(x - \frac{\pi}{4}\right)}{P_1 \cdot \sin\left(x - \frac{\pi}{4}\right) + Q_1 \cos\left(x - \frac{\pi}{4}\right)} = \frac{3}{2x} - \frac{I_0(x)}{I_1(x)} = a \quad (11)$$

or
$$\tan \left(x - \frac{\pi}{4} \right) = \frac{P_0 - aQ_1}{Q_0 + aP_1} \dots \dots \dots (12)$$

Putting $y = \frac{1}{8x}$, $\alpha = 12y - \frac{I_0(x)}{I_1(x)}$

$$= -1 + 8y - 24y^2 - 192y^3 - 2016y^4 - 27648y^5 \dots$$

This value substituted in (12) gives, after simplification,

$$\tan \left(x - \frac{\pi}{4} \right) = -1 - 10y - 10y^2 + 320y^3 + 3650y^4 + 59840y^5 \dots$$

$$\therefore \tan (x - n\pi) = -5y + 20y^2 + 85y^3 + 500y^4 + 21070y^5 \dots$$

Expressing the angle in terms of the tangent, we find

$$x - n\pi = -5y + 20y^2 + \frac{380}{3}y^3 + 20320y^5 \dots$$

or
$$x = n\pi - \frac{5}{8x} + \frac{20}{(8x)^2} + \frac{380}{3(8x)^3} + \frac{20320}{(8x)^5} \dots$$

Then, as before, by Lagrange's theorem,

$$x = \beta - \frac{5}{8\beta} + \frac{20}{(8\beta)^2} - \frac{220}{3(8\beta)^3} + \frac{2400}{(8\beta)^4} + \frac{54560}{3(8\beta)^5} \dots$$

(ii.) When $p=1$ —i.e., when the vibrating plate has one nodal diameter, equation (9A) gives

$$\frac{(6+4x^2)J_1 - (3x+4x^3)J_0}{(6-4x^2)J_1 - 3xJ_0} = \frac{(6-4x^2)I_1 - (3x-4x^3)I_0}{(6+4x^2)I_1 - 3xI_0} \quad (13)$$

This reduces to

$$\tan \left(\frac{\pi}{2} - x + n\pi \right) = 9y - 84y^2 + 639y^3 - 6804y^4 + 168714y^5 \dots$$

Hence
$$x = (2n+1)\frac{\pi}{2} - \frac{9}{8x} + \frac{84}{(8x)^2} - \frac{396}{(8x)^3} - \frac{65261}{(8x)^5} \dots$$

Writing β for $(2n+1)\frac{\pi}{2}$, Lagrange's theorem then gives

$$x = \beta - \frac{9}{8\beta} + \frac{84}{(8\beta)^2} - \frac{1044}{(8\beta)^3} + \frac{18144}{(8\beta)^4} - \frac{385877}{(8\beta)^5} \dots$$

(iii.) The general expression for the roots of equation (9A), as far as the term containing $1/(8\beta)^4$ can be obtained from a result of Kirchhoff's, viz.,

$$\tan\left(x - n\pi - \frac{p\pi}{2}\right) = \frac{B/(8x) + C/(8x)^2 - D/(8x)^3 \dots}{A + B/(8x) + D/(8x)^3 \dots} \dots,$$

where $A = \gamma = \frac{4}{3}$, using Poisson's value of $\mu = \frac{1}{4}$,

$$B = \gamma (1 - 4p^2) - 8,$$

$$C = \gamma (1 - 4p^2)(9 - 4p^2) + 48(1 + 4p^2),$$

$$D = -\frac{\gamma}{3}\{(1 - 4p^2)(9 - 4p^2)(13 - 4p^2)\} + 8(9 + 136p^2 + 80p^4)$$

$$\tan\left(x - n\pi - \frac{p\pi}{2}\right) = -(4p^2 + 5)y + 4(16p^2 + 5)y^2 \\ - \frac{1}{3}(64p^6 + 304p^4 + 1804p^2 - 255)y^3 \dots$$

Writing β for $(2n+p)\frac{\pi}{2}$ and m for $4p^2$, it is easily shown that

$$x = \beta - \frac{m+5}{8x} + \frac{4(4m+5)}{(8x)^2} - \frac{4(m-1)(m+95)}{(8x)^3} + \frac{0}{(8x)^4} \dots \\ \therefore x = \beta - \frac{m+5}{8\beta} + \frac{4(4m+5)}{(8\beta)^2} - \frac{4(7m^2+154m+55)}{3(8\beta)^3} \\ + \frac{96(m+5)(4m+5)}{(8\beta)^4} \dots \quad (14)$$

This series is not convergent enough to give the earlier roots of equation (9A). These can be obtained without difficulty from tables of Bessel functions.

Poisson (*Mém. Acad.*, 1829) found the first two roots of equation (9A), when $p=0$, viz., 2.9815 and 6.1936.

Kirchhoff calculated some of the roots of the general equation (9A) by expressing it in the form

$$0 = 1 - \frac{x^4}{A_1} + \frac{x^8}{A_2} - \frac{x^{12}}{A_3} + \dots$$

and finding the roots by "trial." Only the first two roots, $(\lambda l)^4$, in each case ($p=0, 1, 2, 3$) could be calculated from the table of values of A_1, A_2 , &c., given in the Paper. Kirchhoff's roots are readily expressed in the same form as those calcu-

lated from (14). Several of these values have been verified and are included in the following table :—

TABLE II.
Roots of Equation (9A).
When $p=0, 1, 2, 3.$ $\mu=\frac{1}{4}$ (Poisson's value).

$n.$	$p=0.$	$p=1.$	$p=2.$	$p=3.$
0	2.348	3.571
1	2.982	4.518	5.940	7.291
2	6.192	7.729	9.186	10.600
3	9.362	10.903	12.381	13.821
4	12.519	14.024	15.556	17.015
5	15.669	17.218	18.721	20.203
6	18.817	20.368	21.880	23.363
7	21.963	23.516	25.035	26.526
8	25.108	26.663	28.187	29.685
9	28.253	29.809	31.337	32.841

The values of $\lambda(p=0, \mu=\frac{1}{4})$ can be found from (8)

$$\lambda = \frac{J_0'(x)}{iJ_0'(ix)} = -\frac{J_1(x)}{I_1(x)},$$

and x has one of the values in column 2 of Table II. When $x=\kappa a=9.362$, for example, $\lambda=-0.0001299\dots$ The radii of the nodal circles are readily found from the roots of the equation

$$J_0(\kappa r) - 0.0001299 - J_0(i\kappa r) = 0. \quad \dots \quad (15)$$

The four roots of (15) are 2.40406, 5.5369, 8.3662 and 9.3620. Hence the radius of the plate being taken as unity,

$$\begin{aligned} r_1 &= 0.25679 & 0.25679, \\ r_2 &= 0.59143 & 0.59147, \\ r_3 &= 0.89365 & 0.89381. \end{aligned}$$

For comparison, Kirchhoff's calculated results* are given in the second column.

The roots of equation (9A) vary with the assumed value of μ , but, with the exception of some of the earlier roots, the variations are comparatively small. The value of the roots for any given value of μ can be obtained without difficulty. For example, if $\mu=\frac{1}{3}$ (Wertheim's value),

$$x = \beta - \frac{3m+13}{3(8\beta)} + \frac{40(3m+5)}{9(8\beta)^2} \dots$$

*Strehlke, "Pogg. Ann.," 1855. Lord Rayleigh, "Theory of Sound," Vol. I.

If this expression be divided by the value of x in (14), the new series of roots can be found with considerable accuracy by multiplying the roots in Table II. by this quotient, viz. :—

$$1 + \frac{2}{24\beta^2} - \frac{4\beta(6m-5) - 6(m+5)}{(24\beta^2)^2} \dots$$

TABLE III.
Roots of Equation (9A).
 $\mu = \frac{1}{3}$ (Wertheim's value).

n .	$p=0$.	$p=1$.	$p=2$.	$p=3$.
0	2.292	3.496
1	3.013	4.530	5.936	7.274
2	6.206	7.737	9.188	10.595
3	9.371	10.910	12.386	13.820
4	12.526	14.029	15.559	17.015

The first two roots in each column are those given by Kirchhoff (Crelle, 1850, § 85).

The calculated values of the radii of the nodal circles vary very little for different values of μ . Taking the case where the variation is greatest, viz., when $p=3$ and $n=0$, the change in the value of the radius when μ is changed from $\frac{1}{4}$ to $\frac{1}{3}$ is less than 1 in 500. (Lord Rayleigh, "Theory of Sound," Vol. I., p. 363.)

The change in the calculated value of the frequency of vibration of a "free" plate for a given change in μ is easily found from a consideration of the expression for the frequency, viz. :—

$$N = \frac{ax^2}{\sqrt{1-\mu^2}},$$

a being constant for the same plate and x one of the roots of equation (9A). When the value of μ is changed x also changes and both contribute to the variation in the value of N . The factor $1/\sqrt{1-\mu^2}$ introduces a change of about 2.7 per cent. in the frequency when the value of μ is changed from $\frac{1}{4}$ to $\frac{1}{3}$. Since N varies as the square of the root (x), the variation due to this is easily found from Tables II. and III. For example, when $p=3$, a change in μ from $\frac{1}{4}$ to $\frac{1}{3}$ diminishes the first root by about 2 per cent., and, therefore, the calculated value of the frequency is about 4 per cent. less. In the second mode of vibration, the decrease in the frequency is less than 1 in 200, in the third mode less than 1 in 1,000, and so on.

XXII. *On the Curvature of the Spectrum Lines in a Concave Grating.* By ALBERT EAGLE, B.Sc., A.R.C.S., Imperial College of Science and Technology.

RECEIVED FEBRUARY 10, 1911.

IT is well known that in the case of spectra obtained with prisms or plane gratings the lines are curved and formulæ for the curvature have been given in both cases. It is obvious that since a plane grating is a particular case of a concave grating the spectrum lines must be curved in the latter case also. The astigmatism of a concave grating, combined with any curvature of the spectrum lines, will, it is clear, readily spoil the definition, and it becomes of importance to inquire whether any appreciable resolving power is lost under the conditions of usual practice, and also whether such loss is more detrimental in a new form of mounting described by the author * than in Rowland's mounting.

Lord Rayleigh has recently called attention to the fact that the curvature of the spectrum lines in a concave grating does not appear to have been determined.† I have, therefore, thought it of sufficient interest to publish the following investigation.

Let the rulings of the grating be defined by the intersection of the series of parallel planes $y=ne$, where n is any positive or negative integer, with the sphere

$$x^2+y^2+z^2=2Rx, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

so that the plane $x=0$ is a tangent at the centre of the grating and the rulings are parallel to Oz . Let $P=(a, b, c)$ be a point in the source and $P'=(a', b', c')$ in the image. Let $A=(x, y, z)$ be a point on the surface of the grating. Let P and P' lie on the cylinder

$$x^2+y^2=Rx, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

described on the radius of curvature of the grating as diameter, and having its axis parallel to the rulings. Let y, z, c, c' be small quantities of the first order compared with R ; x will then be a small quantity of the second order.

It is well known that with such gratings as are employed in practice the effect of the fourth order terms is negligible. We will hence omit terms higher than the third order throughout.

* A. Eagle, "On a New Mounting for a Concave Grating," "Astrophysical Journal," XXXI., p. 120, 1910.

† "Encyclopædia Britannica" (eleventh edition), Vol. VIII., p. 249.

We have

$$AP^2 = a^2 + b^2 - 2ax - 2by - 2cz + y^2 + z^2 + c^2.$$

Putting $a^2 + b^2 = \rho^2$, and substituting the value of x from (1) we get

$$AP^2 = \rho^2 - 2by - 2cz + \frac{b^2}{\rho^2}(y^2 + z^2) + c^2,$$

since $1 - a/R = b^2/\rho^2$ by (2).

This may be written

$$AP^2 = \left(\rho - \frac{by + cz}{\rho} \right)^2 + \frac{b^2 z^2 - 2bcyz + c^2 \rho^2}{\rho^2}.$$

Hence

$$\begin{aligned} AP &= \rho - \frac{by + cz}{\rho} + \frac{b^2 z^2 - 2bcyz + c^2 \rho^2}{2\rho(\rho^2 - by - cz)} \\ &= \rho - \frac{by + cz}{\rho} + \frac{c^2}{2\rho} + \frac{b^2 z^2}{2\rho^3} + \frac{bc^2 y}{2\rho^3} - \frac{2bcyz}{2\rho^3} + \frac{b^3 y z^2}{2\rho^5}, \end{aligned}$$

correct to terms of the third order. A similar equation holds for AP' . The optical path $PA + AP'$ must be stationary along the same ruling, hence to find the point A we differentiate with respect to z and equate to zero, which gives

$$(b^2/\rho^3 + b'^2/\rho'^3)z = c/\rho + c'/\rho' \dots \dots \dots (3)$$

Substituting this value of z , the third order terms become terms in y , which have to be added to the main term, and, since the total coefficient of y must be equal to $n\lambda$ c for a spectrum line, we see that b'/ρ' , the sine of the angle of diffraction, must vary for different points in the spectrum line, it being assumed that b/ρ is constant for all points in the source.

Two cases only are of sufficient interest to consider. The first is when the grating is mounted in Rowland's manner, and the light is diffracted along the normal. The second is when the diffracted light returns along the line of incidence as in a mounting recently described.*

In the first case we may put $\rho = R \cos i$, $b = \rho \sin i$, $\rho' = R$, and b' a small quantity of the second order. Equation (3) then becomes

$$b^2 z / \rho^3 = c / \rho + c' / R. \dots \dots \dots (4)$$

Substituting this value of z in the expression for $PA + AP'$ we get, since the coefficient of y is constant,

$$b'/R = \rho'^{1/2} \{ 2bR^2 - (\rho^2 - b^2)c^2 / 2b\rho^3 - (c'^2 - c^2) / 2R^2 \sin i \}. \dots \dots (5)$$

This equation represents a series of parallel arcs, the arcs arising from $+c$ and $-c$ overlapping if c be not too great. It must

* A. Eagle, *loc. cit.*

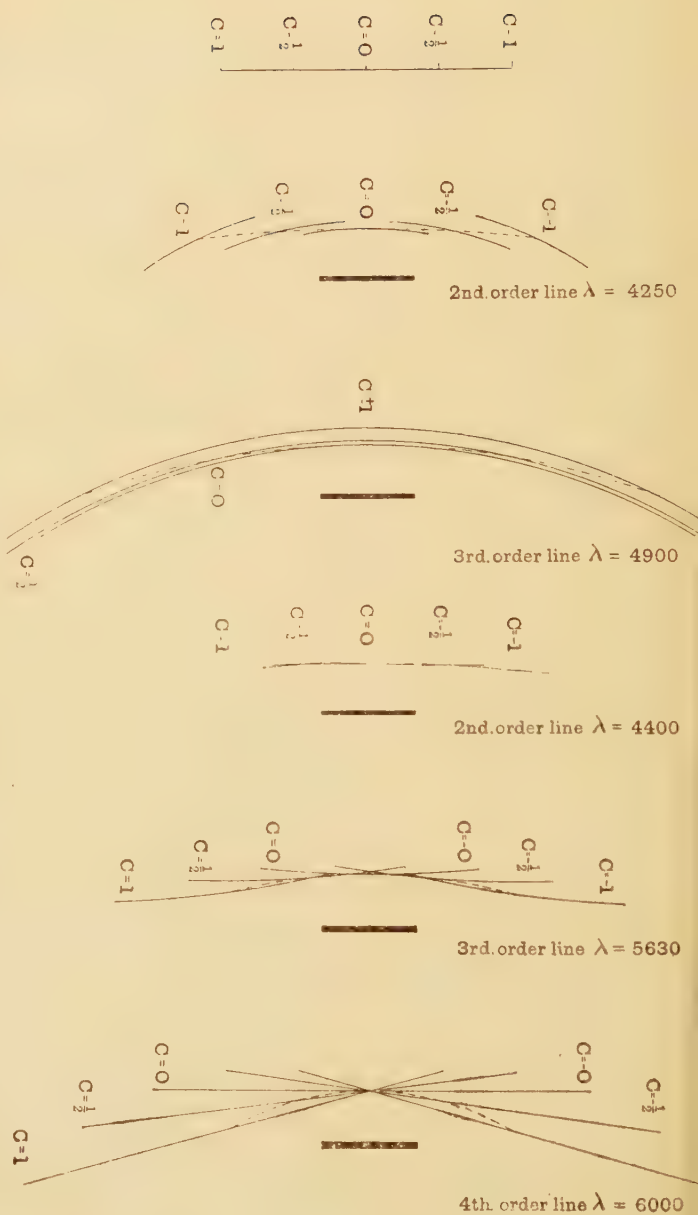


FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

Diagram showing structure of spectrum lines in a concave grating. The continuous black lines represent light coming from five points at $\frac{1}{2}$ in. intervals on the slit. The dotted line is the line joining the centres of the others and to which the spectrum line reduces in a plane grating. The red end of the spectrum is to the right in all cases. The horizontal scale is 50 times the vertical scale.

Fig. 1. Slit, showing the points to which the different arcs correspond.

Fig. 2. Structure with Rowland's mounting $i = 30$ deg.

Fig. 3. " " " $i = 60$ "

Fig. 4. " " " new $i = 15$ "

Fig. 5. " " " " $i = 30$ "

Fig. 6. " " " " $i = 45$ "

The short black lines represent the diffraction width, taken as equal to the distance from the central maxima to the first minima.

be borne in mind that the value of z cannot exceed $l/2$, where l is the length of the rulings. Hence, putting $c=0$, we see from (4) that a point at the slit is drawn out into a line of length $b^2 l R / \rho^3 = l \tan i \sin i$. For a constant value of c , (5) gives an arc of a circle whose radius of curvature $c'^2 / 2b' = R \sin i$. The middle point of each arc corresponds to $z=0$, for which $c = \rho c' / R$ by (4); substituting this value of c in (5) we see that the centres of all the arcs lie on the curve $b' = c'^2 \sin i / 2R$, and is thus an arc of a circle whose radius of curvature is $R / \sin i$. From the sign of b' it can easily be seen that the *concavity* of the lines as well as of the arc on which their centres lie is towards the red.

If this curvature is not seriously to affect the definition, the path error between rays falling on the centre of the grating and one edge must not exceed $\lambda/4$. Hence the displacement b' must not exceed $\lambda R / 4s$, where $2s$ is the width of the ruled space. Putting $c'=0$ we see from (5) that the displacement at the centre of the line is $-c^2 / 2R \sin i$. Hence we must have $c^2 \leq R^2 \lambda \sin i / 2s = R^2 e \sin^2 i / 2ns$, where n is the order of the spectrum. Taking the formula for the astigmatism we see that the centre of the spectrum line will not be illuminated if $2c > l \tan i \sin i$. Applying these conditions to a 6 in., 21 ft. grating, with rulings 3 in. long and 15,000 lines to the inch, we find that the length of the slit illuminated need not be considered in the first order. In the second and third orders the length of slit used should not exceed $0.8 \sin i$ inches ($20 \sin i$ mm.) and $0.7 \sin i$ inches ($17 \sin i$ mm.) respectively. Figs. 2 and 3 show how the spectrum line is built up for angles of incidence of 30 deg. and 60 deg. in the grating considered.

Consider next the case in which the light is diffracted along the angle of incidence. Let $b' / \rho' = b / \rho + \beta / R$, where β is a small quantity of the second order. β is then the lateral displacement of any element of the spectrum line. Equation (3) gives us

$$2b^2 z / \rho^2 = c + c'. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Substituting this value of z in the third order terms for the expression of the total path length and equating the total coefficient of y to a constant, we get

$$\beta = - \frac{(c^2 + c'^2) \cos 2i + 2cc'}{2R \sin 2i \cos i}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

For a constant value of c this gives an arc of a circle whose radius of curvature is $c'^2 / 2\beta = R \tan 2i \cos i$, with its convex

side towards the red if the angle of incidence be less than 45 deg. The length of each of these arcs is obtained by putting $z = \pm l/2$ in (6), which gives $2l \sin^2 i$. To find the locus of the centres of the different arcs we put $z=0$ and, therefore, $c = -c'$ in (7), which then becomes $\beta = c^2 \tan i \sec i/R$, and is thus an arc whose radius of curvature is $R \cos^2 i/2 \sin i$, with its concavity towards the red.

The system of arcs given by (7) for the angles of incidence 15 deg., 30 deg. and 45 deg. is shown in Figs. 4, 5 and 6, which are drawn for the same grating as Figs. 2 and 3.

The displacement at the centre of the spectrum line, *i.e.*, at $c'=0$ is $\beta = -c^2 \cot 2i \sec i/2R$, but it must be borne in mind that light from c will not illuminate the point $c'=0$ if $c > l \sin^2 i$. Considering the same grating as before it is easily found that the definition will never be affected at the centre of the spectrum line in the first order spectrum, while in the second, third and fourth orders the length of slit illuminated should not exceed 1.2, 0.95 and 0.8 in. each multiplied by $\sin 2i \sec^3 2i$, respectively. These restrictions, it can be seen, are not so severe as in Rowland's mounting.

If the slit of a concave grating be rotated in its own plane, the arcs into which different points of the slit are drawn out into in the spectrum will obviously remain parallel to their original direction, while the arc joining their mid points will be rotated through an angle of the same order of magnitude as the slit, hence a very slight rotation will merely shift the point where this latter arc is tangential to the first set (*i.e.*, the point of best definition) up or down the spectrum line. From this we can see that if the slit were entirely below the plane $z=0$ and the spectrum line entirely above it, the structure of the line could be made exactly the same, by turning the slit a little out of the vertical, as if both slit and spectrum line were symmetrical about the plane $z=0$. Thus no resolution is lost by having the slit entirely below the plane $z=0$, which is necessary in the mounting now considered, but the best definition is secured when the spectrum lines are not quite at right angles to the run of the spectrum. This angle for the grating whose mounting I have described is about $0.6 \tan i \sec i$ degrees. Angles of incidence greater than 45 deg. are not used in this mounting, since an angle of incidence of 30 deg. will give the same position in the spectrum as an angle of 90 deg. in Rowland's mounting. Hence this inclination never exceeds 1 degree.

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